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RANK ORDER AS A PSYCHOPHYSICAL METHOD

BY L. L. THURSTONE

University of Chicago

In every psychophysical experiment it is necessary for the investigator to select a psychophysical method and in doing so he is guided by two principal types of consideration, namely experimental convenience and theoretical propriety. Other things being equal, he will choose that experimental procedure which is simplest for the subject and most convenient. He will also be constrained to select a method which is legitimate for his problem. One of the simplest of experimental procedures is to ask the subject to arrange a series of stimuli in absolute rank order according to length, weight, brightness, beauty or whatever the psychological continuum may happen to be. This experimental procedure does not ordinarily lend itself to psychological measurement because the subjective increments represented by the successive rank orders are ordinarily entirely unknown. Occasionally it is legitimate to assume that the entire distribution of subjective values is Gaussian and then it is possible to translate rank orders into legitimate psychological measurement. We are dealing now with the situation in which a small number of stimuli, such as ten or twenty, are to be dealt with and in which no assumption can be made legitimately regarding their distribution in subjective value.

For theoretical purposes the constant method is probably the best in most experimental problems. The ideal form of the constant method is the method of paired comparison in which not one but all of the stimuli serve as standards. But

the constant method is also one of the most laborious experimental methods. If there are twenty stimuli and if the method is to be complete there would be required 190 judgments in order to compare each stimulus with every other stimulus. This assumes that the constant method is used in complete form with every stimulus serving in turn as a standard. The method has serious limitations when only one or two of the stimuli are used as standards. We shall also assume that the intermediate category is excluded. If the reader insists on using the intermediate category of judgment in the constant method, this paper is of no interest to him.

Our present problem is to devise a plan whereby simple absolute rank order may be used as the experimental procedure with the advantages of the much more laborious constant method. Given the data for absolute rank order we shall extract the proportion of judgments "A is greater than B" for every possible pair of stimuli in the given series. These derived proportions will be used instead of the proportions that are obtained directly in the constant method. From these derived proportions the subjective separations between any pair of stimuli can then be readily calculated by the equation of comparative judgment. The method will be derived first theoretically and then we shall describe its empirical verification.

If a subject has placed four stimuli *A B C D* in the rank order *B D A C* it is possible to tabulate his various comparisons as though he had made them separately. If each of the four stimuli were to be compared with every other one in the series it would require six separate judgments, namely *AB AC AD BC BD CD.* If there are n stimuli in the series it would require $n(n-1)$ such judgments with counterbalanced order of presentation or half that many if counterbalanced order is disregarded. This would give only one judgment for each of the possible pairs of stimuli. Now if the four stimuli have been placed in the rank order *B D A C* by one subject, it is clear that six judgments may be extracted from this one rank order. Evidently the above rank order series is equiva-

lent to the judgments $B > D$, $B > A$, $B > C$, $D > A$, $D > C$, $A > C$. If a large number of subjects have arranged fifteen or twenty stimuli in rank order it is an almost prohibitive task to tabulate the separate judgments to which the single rank order is equivalent. However, it can be done by a shorter procedure.

Let there be n specimens in the series to be arranged in rank order by N subjects. Let A and B be two of these specimens and let a_1 = frequency with which specimen A is placed in rank 1 by the N subjects, b_1 = frequency with which specimen B is placed in rank 1 by the N subjects, p_{a1} = proportion of the N subjects who place specimen A in rank 1, p_{b1} = proportion who place specimen B in rank 1, and similarly for the other specimens and the other rank orders. See *figure 1*.

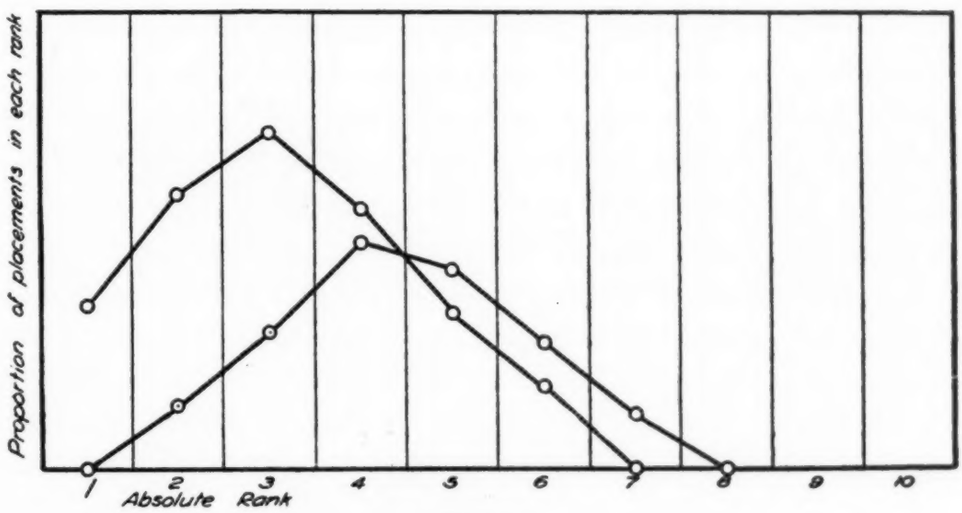

FIG. 1

Since these values of p may be regarded as probabilities, we have $p_{b2} + p_{b3} + p_{b4} + \ldots p_{bn} = p_{b>1}$ = probability that any subject at random (or any one judgment of a single subject) will place B in a rank higher than rank 1.

Hence $p_{a1} \cdot p_{b>1}$ = probability that any subject, chosen at random, will place A in rank 1, and B in a higher rank. Similarly, $p_{a2} \cdot p_{b>2}$ = probability that a subject will place A in rank 2 and B in a higher rank.

In general, this product may be written $p_{ak}\ p_{b>k}$ =

probability that A will be perceived in rank k and that B will be perceived in a rank higher than k.

Summing for all of the n ranks, we have $\Sigma(p_{ak}\, p_{b>k}) =$ probability that B will be perceived in a rank higher than that of A.

But we must also consider the possibility that the two specimens will be perceived as of practically equal or nearly equal rank. If it were possible for two specimens to be perceived in the same rank order, then $p_{ak}\, p_{bk} =$ probability that both specimens A and B will be perceived in the same rank order k. But we assume that the subject is asked to place all the n specimens in absolute rank order without any duplicate or tied ranks and hence it is experimentally impossible for the two specimens to be placed in the same rank order. We shall make the assumption that if the two specimens are perceived to be sufficiently nearly alike to warrant the same rank order, the probability $p^1_{a>b} = .50$, and the probability $p^1_{b>a} = .50$. The notation p^1 refers to a single class interval. This is not quite correct because we know that if the two stimuli are slightly different in objective measurement then there will be a slight majority of correct judgments while the incorrect judgments will be in the minority.

This, of course, ignores the possible time and space errors. This is legitimate in dealing with rank order as an experimental method because the subject is given a series of stimuli or specimens to sort out into a rank order by his own devices. It is therefore a matter of chance which of any pair of stimuli is perceived first and whether it happens to be held to the right or to the left of the second stimulus. The subject has the privilege of revising his results and of looking at any and all of the specimens in any order and as many times as he may choose. The problem of constant errors may therefore be ignored.

However, if the two specimens differ slightly in psychological value so that $A > B$ and if they are presented to a subject repeatedly or once to a group of subjects in counterbalanced order by the constant method, we should find that $p_{a>b} > .50$. In other words, a slight majority would favor specimen A. The departure of the judgment $p_{a>b}$ from .50

will be small if the difference between A and B is small. Since in a rank order experiment we shall assume that n is as large as 10 or 15 or 20, the interval in value represented by one rank order is relatively small. In such situations and especially when the discriminal error is much larger than the interval represented by one rank order, our assumption is approximately correct, namely that

$\frac{p_{ak} \cdot p_{bk}}{2}$ = probability that both A and B will be perceived in the same rank order interval and that B will be perceived higher than A.

Hence we may write the formula with this approximation as

$$p_{b>a} = \Sigma(p_{ak} \cdot p_{b>k}) + \tfrac{1}{2}\Sigma(p_{ak} \cdot p_{bk}). \qquad (1)$$

In other words, we have expressed the proportion of subjects who perceive B higher than A in terms of the frequencies with which the two specimens are placed in the n rank orders. We can now use simple absolute rank order as an experimental procedure and we can obtain the same results as with the order of merit method and practically the same results as with the constant method with counterbalanced order. It is taken for granted here that the intermediate category is not used.

The approximation involved in the last term of equation (1) is close enough for all situations in which the number of stimuli to be arranged in rank order is greater than 10 or 15. The approximation is not satisfactory when the number of stimuli is small such as five or six. In the present study we have used the approximation represented by equation (1) but we shall develop here a more general formula for translating rank order into the proportions of the constant method which can be used when the experiment involves only a small number of stimuli.

If the number of stimuli represented in *figure 1* is rather small it is evident that some distortion is introduced by regarding the probability ordinates to be constant within each class interval. That is in effect the assumption in

deriving the approximation equation (1). That equation is derived as though the diagrams in *figure 1* were drawn as column diagrams instead of probability polygons. Let *figure 2* represent one of these class intervals in which the probabilities show variation within the class interval. The probability that stimulus 1 will be perceived in this class interval is p_1, and the notation p_2 has a similar interpretation for the second stimulus. The horizontal dotted lines in *figure 2* represent the situation in which a stimulus would be

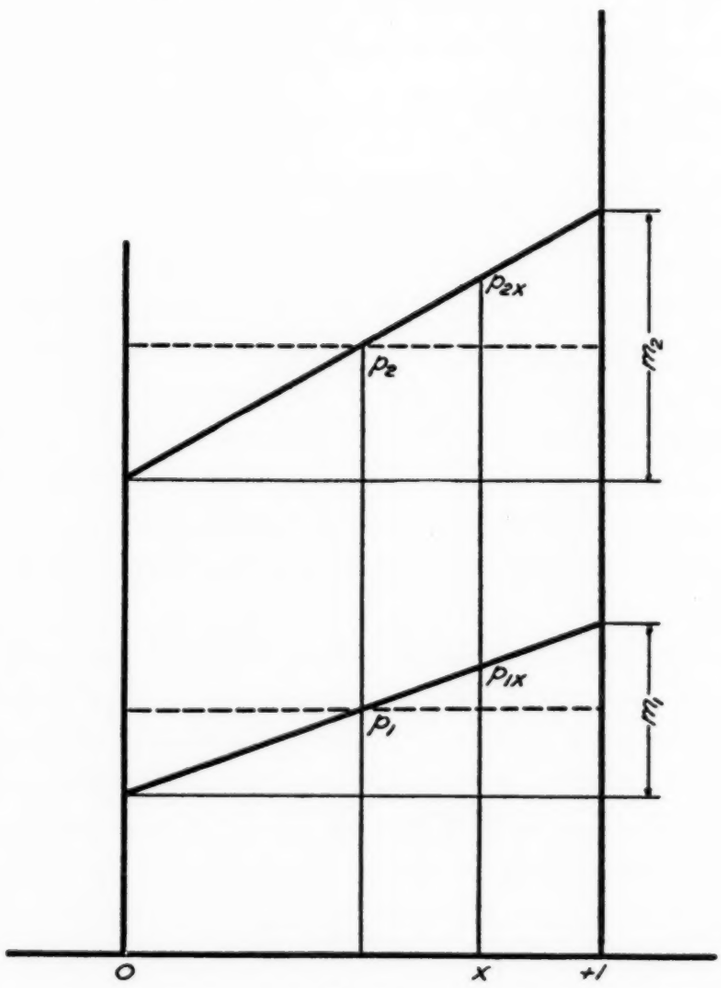

FIG. 2

as likely to be perceived at one part of the class interval as at any other part of it. Let the sloping straight line through p_1 represent for each value of x the probability that stimulus 1 will be perceived at x. As we have drawn *figure 2*, the

stimulus is more likely to be perceived in the upper part of the class interval than in the lower part of it but our correction formula will cover the general case in which the probability is assumed to vary throughout the class interval according to any linear function.

By inspection of *figure 2* we see that the equations of the two sloping straight lines are as follows:

$$p_{1x} = m_1 x + p_1 - \frac{m_1}{2}, \tag{2}$$

$$p_{2x} = m_2 x + p_2 - \frac{m_2}{2}, \tag{3}$$

in which

p_{1x} = the probability that stimulus 1 will be perceived at any point x within the class interval.

p_{2x} = the probability that stimulus 2 will be perceived at any point x within the class interval.

m_1 and m_2 are the slopes of the lines.

p_1 and p_2 are the probabilities that stimuli 1 and 2 respectively will be perceived in the class interval.

The probability P_{1x} that stimulus 1 will be perceived higher than x but within the class interval is therefore

$$P_{1x} = \int_x^{+1} \left(m_1 x + p_1 - \frac{m_1}{2} \right) dx. \tag{4}$$

The probability that stimulus 2 will be perceived at x and that stimulus 1 will be perceived higher than x but within the class interval is the product of these probabilities, namely,

$$\begin{aligned} p^1{}_{1>2} &= \int_0^{+1} p_{2x} \cdot P_{1x} . dx \\ &= \int_0^{+1} \left[\left(m_2 x + p_2 - \frac{m_2}{2} \right) \int_x^{+1} \left(m_1 x + p_1 - \frac{m_1}{2} \right) dx \right] dx. \end{aligned} \tag{5}$$

After integrating and simplifying, we have

$$p_{1>2}{}^1 = \frac{p_1 p_2}{2} + \frac{1}{12} (m_1 p_2 - m_2 p_1). \tag{6}$$

Inspection of equation (6) makes it evident that if the two slopes m_1 and m_2 are zero, this term becomes identical with the second term of equation (1) which is what we should expect. Furthermore, if a and b are interchanged as well as p_1 and p_2 we should have

$$p_{1>2}{}^1 + p_{2>1}{}^1 = p_1 p_2. \tag{7}$$

In other words, the probability that both stimuli will be perceived in the same class interval is $p_1 p_2$. This probability is split into two parts, namely the probability $p_{1>2}{}^1$ that 1 is perceived above 2, and the probability $p_{2>1}{}^1$ that 2 is perceived above 1 *in the same class interval.*

If we use equation (6) in summation form instead of the second term of equation (1), we shall have a closer approximation to the true value of $p_{1>2}$. Let the class interval of *figure 2* be designated k and let the two stimuli 1 and 2 in *figure 2* be designated b and a respectively, so that p_2 and p_1 in that figure become, in the more general notation, p_{ak} and p_{bk} respectively. Then the complete formula becomes

$$p_{b>a} = \Sigma(p_{ak} \cdot p_{b>k}) + \tfrac{1}{2}\Sigma(p_{ak} \cdot p_{bk}) + \frac{1}{12}\Sigma(m_{bk} \cdot p_{ak} - m_{ak} \cdot p_{bk}), \tag{8}$$

in which the slopes m_{ak} and m_{bk} are defined as follows:

$$m_{ak} = \frac{p_{a(k+1)} - p_{a(k-1)}}{2} \text{ and } m_{bk} = \frac{p_{b(k+1)} - p_{b(k-1)}}{2}. \tag{9}$$

The notation in the general equation (8) may be summarized as follows:

$p_{b>a}$ = estimated proportion of subjects who judge stimulus b higher than stimulus a.

p_{ak} = proportion of subjects who place stimulus a in any particular rank order k. Similar interpretation for p_{bk}.

$p_{a>k}$ = proportion of subjects who place stimulus a higher than any specified class interval k. Similar interpretation for $p_{b>k}$.

$p_{a(k+1)}$ = proportion of subjects who place stimulus *a* in the rank order next higher than *k*. Similar interpretation for $p_{b(k+1)}$.

$p_{a(k-1)}$ = proportion of subjects who place stimulus *a* in the rank order next lower than *k*. Similar interpretation for $p_{b(k-1)}$.

It should be noted that both of the equations (1) and (8) are approximation equations but that equation (8) involves the least assumptions. Thus equation (1) assumes that the probabilities in *figure 1* can be adequately represented as column diagrams. This is legitimate for most problems where the number of stimuli is as large as, say 20. Equation (8) assumes only that the variation in the probabilities of each class interval in *figure 2* is linear. It is represented by frequency polygons in *figure 1* instead of column diagrams. In most experimental situations formula (1) is adequate since it shows only a very slight discrepancy with the actual count for $p_{b>a}$.

Returning to formula (1) we shall now show its application to some experimental data. The psychophysical comparisons of social stimuli are much more complex than the comparisons of simple sensory stimuli such as line lengths and weights. If our method is applicable to the complexities of social stimuli, they may safely be assumed to be applicable to the simpler case of sensory stimuli. We shall test the formula on Miss Hevner's data for judgments about handwriting specimens.[1]

In her experiment on the order of merit method she asked 370 subjects to arrange twenty specimens of handwriting in rank order. From such experimental data it was of course possible to count the number of subjects who placed each one of the twenty specimens in each one of the twenty rank orders. For example, 59 out of the 370 subjects placed specimen 4 in the fifth place from the top in excellence. In a table of this kind there must of course be as many rank orders as there are specimens because tied ranks were not allowed. From this table a second table was prepared showing the

[1] Kate Hevner: *A Comparative Study of Three Psychophysical Methods.* Doctor's Thesis, The University of Chicago.

TABLE 1

PROPORTION OF 370 JUDGES WHO PLACED EACH ONE OF TWENTY HANDWRITING SPECIMENS IN EACH OF TWENTY ABSOLUTE RANK ORDERS BY THE ORDER OF MERIT METHOD. TWENTY SPECIMENS

Rank Order	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	.3108	.1081	.0676	.0838	.1973	.1595	.0676	.0027	.0027											
2	.2027	.0865	.1297	.1351	.1378	.1838	.1054	.0027	.0081			.0081								
3	.1649	.1135	.1595	.1270	.1324	.1162	.1595	.0054	.0189			.0027								
4	.0919	.1405	.1514	.1378	.1351	.1081	.1595	.0162	.0459	.0027	.0081	.0027								
5	.0784	.1135	.1135	.1595	.1027	.1432	.1784	.0135	.0649	.0000	.0000	.0270		.0027	.0027					
6	.0757	.1432	.1054	.1486	.1162	.1054	.1027	.0568	.1108	.0054	.0054	.0216		.0000	.0027					
7	.0378	.1378	.1000	.1135	.0784	.1027	.1459	.0703	.1405	.0027	.0081	.0486	.0054	.0000	.0054	.0027				
8	.0270	.0703	.0676	.0622	.0703	.0378	.0459	.1405	.2892	.0054	.0297	.1243	.0027	.0054	.0189	.0027				
9	.0054	.0243	.0486	.0189	.0162	.0189	.0243	.2378	.1973	.0432	.0811	.2243	.0162	.0108	.0324	.0000				
10	.0027	.0297	.0324	.0027	.0081	.0162	.0027	.2135	.0676	.0757	.1216	.2649	.0351	.0108	.1081	.0000	.0027	.0054		
11	.0000	.0135	.0162	.0081	.0000	.0054	.0054	.1000	.0270	.1730	.2486	.1189	.0676	.0243	.1892	.0000	.0027	.0000		
12	.0027	.0081	.0027	.0000	.0027	.0027	.0000	.0703	.0216	.2622	.1892	.0811	.1189	.0568	.1757	.0054	.0000	.0000		
13		.0027	.0054	.0027	.0000		.0027	.0405	.0000	.2054	.1757	.0486	.1703	.1270	.1838	.0081	.0135	.0135		
14		.0081			.0000			.0189	.0054	.1162	.0919	.0162	.2946	.2135	.1703	.0216	.0108	.0270	.0027	.0027
15					.0027			.0108		.0730	.0270	.0027	.1973	.3162	.0784	.0351	.1081	.1324	.0081	.0081
16										.0216	.0081	.0054	.0622	.1297	.0189	.1541	.2892	.2541	.0459	.0108
17										.0135	.0054	.0027	.0216	.0676	.0081	.2054	.3162	.2216	.0892	.0486
18													.0081	.0270	.0054	.2757	.1541	.2784	.1243	.1270
19														.0054		.1649	.0811	.0514	.4108	.2865
20														.0027		.1243	.0216	.0162	.3189	.5162

proportion of all the subjects who placed each specimen in each of the twenty rank orders. In the above example this proportion is 0.1595. This means that about sixteen percent of the entire group of 370 subjects placed specimen 4 in rank order five. This is shown in *Table 1* and the rest of the table is interpreted in the same manner.

TABLE 2

Rank Order	Strip No. 1		Strip No. 2			
	p_{1k}	$p_{1>k}$	p_{2k}	$p_{2>k}$	$p_{1k} \cdot p_{2>k}$	$p_{1k} \cdot p_{2k}$
1	.3108	.6894	.1081	.8919	.2772	.0336
2	.2027	.4865	.0865	.8054	.1633	.0175
3	.1649	.3216	.1135	.6919	.1141	.0187
4	.0919	.2297	.1405	.5514	.0507	.0129
5	.0784	.1513	.1135	.4379	.0343	.0089
6	.0757	.0756	.1432	.2947	.0223	.0108
7	.0378	.0378	.1378	.1569	.0059	.0052
8	.0270	.0108	.0703	.0866	.0023	.0019
9	.0054	.0054	.0243	.0623	.0003	.0001
10	.0027	.0027	.0297	.0326	.0001	.0001
11	.0000	.0027	.0135	.0191	.0000	.0000
12	.0027	.0000	.0081	.0110	.0000	.0000
13			.0027	.0083		
14			.0081	.0002		
15						
16						
17						
18						
19						
20						

$$\Sigma(p_{1k} \cdot p_{2>k}) = .6705$$
$$\tfrac{1}{2}\Sigma(p_{1k} \cdot p_{2k}) = .0549$$
$$p_{2>1} = .7254$$

Table 2 is a summary of the calculation for estimating the proportion of subjects who perceived specimen 2 to be better than specimen 1. The first column is a list of the twenty rank orders. For each specimen a strip was prepared similar to the second and third columns. Columns four and five in *Table 2* represent such a strip for the second stimulus. These strips were used in calculation so as to avoid unnecessary transcription. The sixth column shows the product $p_{1k} \cdot p_{2>k}$ for each of the rank orders. This is merely the product of columns two and five in the table. The last column shows

the product $p_{1k} \cdot p_{2k}$ for each of the rank orders. It is the product of items in columns two and four. In actual calculation the entries in the last two columns were not recorded. The products were calculated on a Marchant calculating machine and they were allowed to total without recording of the separate items in the last two columns. The entries of these two columns are here shown for completeness although in practice it is not necessary to write them. Only the sums for the last two columns are recorded. These sums are shown at the bottom of the table. The simple calculation of the estimated proportion of all the subjects who perceived specimen 2 to be better than specimen 1 is also there indicated. It is done in accordance with equation 1.

This procedure was carried out for each of the $\frac{1}{2}n(n-1) = 190$ possible pairs of stimuli although it should be noted that when the two specimens are far apart in excellence the amount of overlapping is small so that the calculation is then short. For example, the strip for specimen 1 covers the first 12 rank orders as shown in the second and third columns of *Table 2*. The corresponding strip for specimen 20 covers the rank orders from 14 to 20 inclusive. Since there is no overlapping it is clear that none of the subjects regarded specimen 20 as better than specimen 1 and consequently we can write without further calculation that the estimated proportion of the subjects who perceived specimen 1 as better than specimen 20 is unity. The amount of calculation is a maximum when the two specimens are nearly of the same degree of excellence and two such specimens are shown in the example of *Table 2*. Consequently the labor of calculation is not nearly so great as would be indicated by merely multiplying the labor of *Table 2* by 190.

Miss Hevner actually tabulated the number of judges who placed each one of the twenty specimens higher than every other specimen. This was accomplished by her records of order of merit for 370 subjects. This was an exceedingly laborious procedure but it was done in order to compare the order of merit method with two other psychophysical methods, namely the method of equal appearing intervals and the

method of paired comparison. In order to test our equation 1 we listed the proportions estimated by equation 1 and also the actual proportions tabulated by Miss Hevner for the order of merit method. The discrepancy for each proportion was listed in the form $(p_H - p_C)$ in which p_H refers to the actual proportion found by Miss Hevner by the order of merit method and p_C refers to the estimated proportions calculated by our present equation (1). The distribution of discrepancies is shown in *figure 3*. The average discrepancy, disregarding

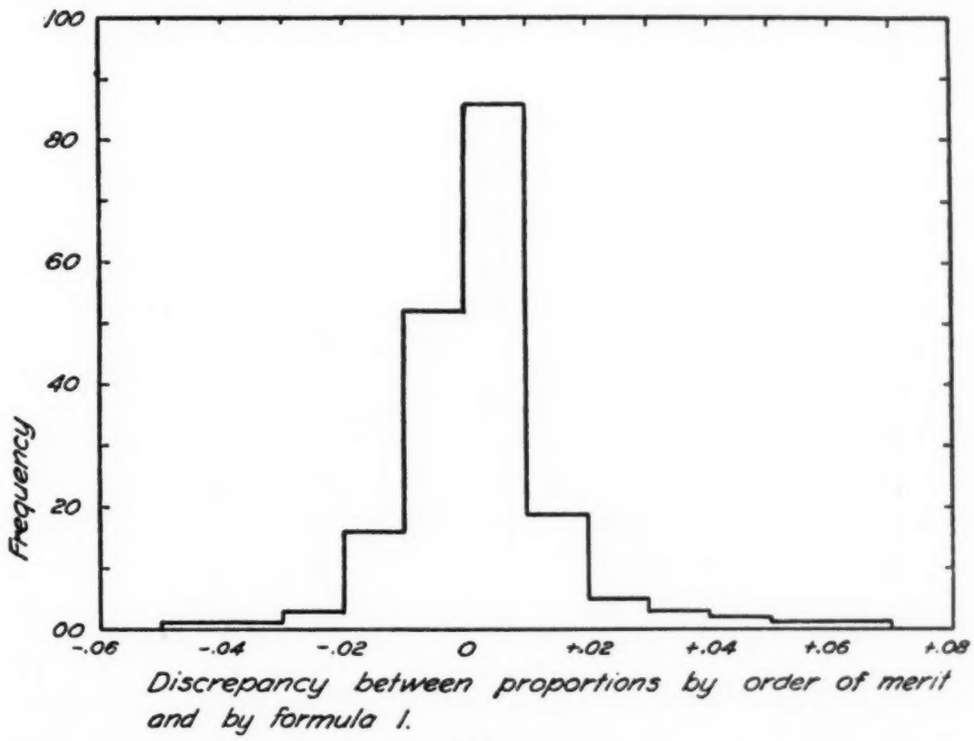

FIG. 3

sign, is .0078 which shows a very close agreement. The fact that the average discrepancy is less than one percent constitutes practical justification for equation (1) as a method of estimating the proportions of the constant method when the experimental procedure was that of simple rank order. The close similarity in results from the order of merit method and the constant method was demonstrated by Miss Hevner.

It is to be expected that the scale values for the twenty specimens determined by the two sets of proportions should be practically identical and this is shown in *figure 3*. We

have tabulated the scale values determined by Miss Hevner for the order of merit method and for the same raw data treated by the present formula. The agreement is practically perfect as shown in *figure 4*. Not only are the two scales comparable but the units are identical since the proportions themselves agree and consequently the slope of the plot in *figure 4* is unity.

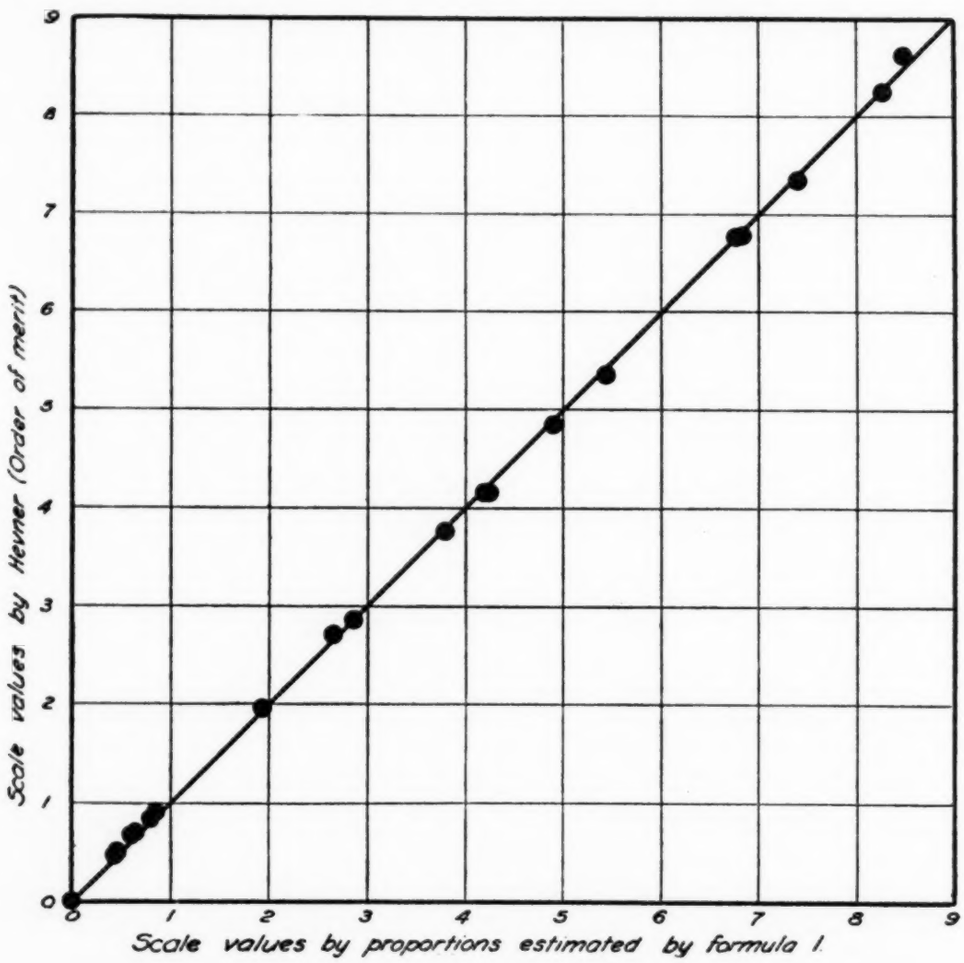

FIG. 4

An important psychophysical inference may be drawn from these experiments. In a previous paper I have assumed that the correlation between discriminal errors of two specimens that are being compared is zero.[2] That is a fundamental assumption of the law of comparative judgment. At that time I did not see any clear method of testing that assumption separately from the other assumptions involved in the same

[2] A Law of Comparative Judgment; *Psychol. Rev.*, 1927, *34*, 273–286.

study. Since the equation of comparative judgment has been shown to fit experimental data for a wide variety of stimuli I have felt assured that the assumptions were justified. In the present data we find, however, a specific verification for the assumption that the correlational term of the equation of comparative judgment is zero. If it were not zero, then the probabilities involved in equation (1) would not be independent and consequently the product of the several probabilities would not tally with the experimentally observed proportion of judgments that constitute a compound event. Since the equation satisfies the experimentally observed frequencies with a degree of accuracy that is unusual in psychological work, we are justified in concluding that the several probabilities are truly independent and that therefore the correlational term in the law of comparative judgment is zero as has been previously assumed.

In Miss Hevner's study a comparison was made between the order of merit method and the constant method in its complete form, namely paired comparison. These two methods were shown to be identical when treated by the equation of comparative judgment. We have here shown that it is not necessary in the order of merit method to tabulate separately all of the $n(n-1)$ judgments for each subject that are implied in his arrangement of n stimuli in a single rank order. It is possible to estimate the proportions directly from a frequency table of rank orders for each specimen. This makes it possible to use simple rank order when that method is experimentally the easiest and to extract from the rank order data the proportions that would be obtained by the laborious constant method or the even more laborious paired comparison method. Miss Hevner has previously shown that the order of merit method gives results practically identical with the constant method. We have also verified our previous assumption that the discriminal errors in a comparison of two stimuli are usually uncorrelated. This assumption enters the law of comparative judgment when the correlational term is assumed to be zero.

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THE *TAU* EFFECT: AN EXAMPLE OF PSYCHOLOGICAL RELATIVITY

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The physical theory of relativity teaches that space and time are inextricably connected, but the best that the physicist can do with this relationship is to make it abstractly intelligible. Psychologically, however, it is possible to make the dependence of space upon time a matter of direct experience in a very simple way as first pointed out by Benussi in vision and Gelb in touch.[1] It is done by exposing three stimuli in succession at equal spatial intervals but unequal temporal intervals. If the time between the first and second stimuli is shorter (longer) than that between the second and third stimuli, the extents undergo a corresponding change, for the first appears shorter (longer) than the second. This dependence of space upon time has been called the *Tau* effect [2] in order to avoid the use of names which might imply that we are dealing with an 'illusion', error in judgment, some effect of attention or suggestion. The objectivity of the phenomenon is established by the fact that it has been found in vision, hearing and touch by different investigators, and is susceptible to quantitative treatment. It appears and persists even when Os are aware of its nature and the conditions responsible for it.

In this paper we shall confine ourselves to the *Tau* effect upon the skin, leaving out of account the phi-phenomenon which appears when the temporal intervals are sufficiently short for apparent movement. Scholz has shown that there is an intimate relation between the time of maximal shortening

[1] V. Benussi, *Psychologie der Zeitauffassung*, 1913, 285ff.; A. Gelb, *Ber. ü. d. 6 Kongress f. exp. Psychol.*, 1914, 36–42. We are greatly indebted in this study to the short report of Gelb. We have adopted his symbols for space and time. *Suum cuique.*

[2] By the senior author, *Science*, 1930, 71, 536–7.

and apparent movement. But since no exact measurements have been made of the effect of temporal interval upon spatial interval without the complicating factor of the phi-phenomenon, which also appears when the time intervals are short, we decided to confine our study to the more restricted question.[3] Gelb, whose results we have fully confirmed, gives no quantitative evidence for his conclusions. It is our aim, therefore, to investigate the dependence of spatial extent upon time without the complicating factor of apparent movement. While quantitative evidence is here presented to support the laws of this dependence, first stated by Gelb, this study can be regarded as only a beginning in this direction.

Apparatus and Procedure. A modified form of the Benussi and Dallenbach kinohapts was used.[4] The arm was held in a plaster of paris cast placed under the main carriage of the kinohapt. This carriage controlled the gross movements of the three stimuli and their appurtenances. The carriage was controlled horizontally by rack and pinion from two knurled knobs permitting a movement of 13 inches, while the vertical movement was controlled by means of a crank within a range of 12 inches. The individual stimulus points were independently variable laterally, longitudinally, and vertically, so that fine adjustment was possible with each of the stimuli separately. In addition, the bases to which the stimuli were attached could be swung on their vertical stems around the circumference of a circle having a diameter of three inches. Six spatial adjustments were thus possible for the three dimensions of space.

The pressure stimuli consisted of blunt fibre tipped rods mounted with a second rod passing through the center of a solenoid.[5] The stimulus fell to the skin when the current

[3] W. Scholtz, Exp. *Unters. ü. d. phänomenale Grösse von Raumstrecken, etc., Psych. Forsch.*, 1924, 5, 219ff., esp. 259ff. An account of Scholz's work will be found in H. Helson, The Psychology of *Gestalt*, *A. J. P.*, 1925, 36, 513f.

[4] Described in *Ninth International Congress of Psychology*, Proceedings and Papers, by S. H. Bartley and H. Helson, p. 67. Our thanks are due to Mr. Bartley who built the apparatus.

[5] We are indebted to Professor E. G. Boring for having communicated the essentials of the solenoids used with the Harvard kinohapt. A similar device has been described recently by W. Hulin, A simplified electromagnetic æsthesiometer, *A.J.P.*, 1929, 41, 476–7.

through the solenoid was broken and raised when the current was made. The resting period upon the skin in all experiments was close to 100 sigmæ. The average weight of the stimuli was 11.54 grams and the height of fall 2 mm. By means of small weights, the rods were made subjectively equal so that intensity differences would play no part in the results.

Timing was controlled by means of a revolving metal disc with contact brushes, driven by an electric motor and reducing gears. A slitted paper disc upon the metal disc allowed the contact brushes to make the current through the metal disc and the solenoids when desired. By cutting the paper discs to suit the speed of the driving mechanism, the resting time of each stimulus and the temporal interval between stimulations could be accurately controlled. The order of stimulation, proximal to distal, or vice versa, was easily managed by simply changing the position of the slits in the discs. The lag in the solenoids was of the small order of 35 sigmæ and so constant as to be negligible.

Eight Os served as regular subjects throughout the experiments: W (Dr. R. H. Wheeler, professor of psychology); B (Mr. S. Howard Bartley, graduate student and instructor in psychology); N (Mr. Edwin B. Newman, graduate student in psychology); L (Mr. M. Howell Lewis, graduate student in psychology); Lew. (Miss V. L. Lewis, a sophomore then taking her first course in psychology); C (Mr. Earl Chambers, a sophomore in the college then taking his first course in psychology and blind since he was five years of age. He had some visual imagery but reported none in this series of experiments. It should be noted that his results are not significantly different from the rest of the Os and that this should be an adequate answer to theories which might attempt to explain our findings by means of visual images or visuomotor habits);[6] finally, the two authors served also as Os,

[6] We do not mean to say that visual imagery did not play a part in the processes responsible for the judgment of distance. The introspections of our Os show that visual imagery of the arm, the spots stimulated and the distance between was often present. Strains and kinæsthetic patterns were also reported. But these introspective by-products appear to be neither necessary nor sufficient for the *Tau* effect and since we are not interested in introspective analysis they may well be passed with this mention

each acting as E for the other. With the exception of H and K (the authors), none of the observers knew the nature of the experiments, their purpose or the exact conditions. While it was inevitable that they should soon discover that the time intervals were different and that this affected their judgment of space, nevertheless by frequent change of spatial and temporal intervals and reversing the effect, so to speak, at any one time it is safe to say that the Os were following instructions as told.

The right forearm of each O was used, the dorsal surface with some, the ventral with others. Both proximal and distal regions were used and the order of succession changed. Since none of these factors, order of stimuli, dorsal or ventral surface, was found to introduce any significant changes, they will not be mentioned again in this study. The arms were shaved in order to avoid the complication arising from stimulation of pressure spots by hairs. Since both spatial and temporal intervals were changed in irregular order, as well as the order of succession of stimuli, the Os were unaware of the essential conditions responsible in a given series of observations for what was experienced and reported.

The instructions to the Os simply called for a report as to whether the spatial interval between the second and third stimulations was equal to, greater, or less than that between the first and second stimulations. The report always refers to the second spatial interval.

Results.—The numbers in the tables refer to the percentages of shorter (S), equal (E), and longer (L) judgments. In every case the judgment was made on the basis of the second *spatial* interval as compared with the first. We shall refer to the judged spatial interval as Sj, the first spatial interval as s_1, the second spatial interval as s_2, the first tem-

of their presence. It is significant that our blind subject who had no visual imagery in these experiments, and little in everyday life, showed no peculiarities. In fact scrutiny of his results in each table shows him to have been a very good observer. Our results and the history of the whole attempt to do anything with any sort of judgmental process through the method of introspective analysis justify us in refusing to burden the literature with reports of process that shed no further light on the phenomenon studied. But we have the protocols if anyone wishes them.

poral interval as t_1, and the second temporal interval as t_2. The standard distance for s_1 was 30 mm. Beginning with s_1 and s_2 equal, *i.e.*, each 30 mm we lengthened or shortened s_2 in 5 mm steps until it was correctly reported without error in 20 consecutive trials. Obviously, if t_2 was less than t_1, we had to lengthen s_2 in order to overcome the *Tau* effect, and if it was greater, we had to shorten s_2. In the bulk of the experiments reported here, t_1 was kept at 500 sigmæ and t_2 *which had* an *initial value* of 200 sigmæ in the first set of experiments (Table 2) was increased in increments of 50 sigmæ. Only one set of experiments showing the "counter-effect," *i.e.*, t_1 less than t_2, is reported at this time, (Table 7).

While it would have been possible to work out the limens for any given distances and temporal intervals of S, E, and L, we have preferred to present the actual data in this first quantitative study of the phenomenon. Moreover, we believe it desirable to give the spatial distances required to offset the effect of the time difference in order to show the full extent and strength of the *Tau* effect. By giving the distances at which the *Tau* effect *completely* disappears, that is, the 100 percent point, we show how compelling it is. That it appears whether we lengthen or shorten the second temporal interval is seen by comparing Table 7 with the preceding tables.

It will be noticed that nearly all the tables show inversions, *i.e.*, whereas we expect the percentage of judgments within a given category to show progressive increase or decrease, we often find an increasing series interrupted by a smaller percentage, or a decreasing series broken into by a number larger than its neighboring values. The fact that inversions are characteristic of all psychophysical functions, at least in the raw data, shows that the *Tau* effect conforms in this irregularity. Our inversions are strictly comparable to the situation in lifted weights where a heavier stimulus gives a larger percentage of 'lighter' judgments than the stimulus preceding and following it. Nevertheless, the general tendencies discernible in these data show that we are dealing with a true psychophysical function. Examples of inversions will be

found, *e.g.*, in Table 2, for C, N, and W; in Table 3, for B, N, Lew., and W, not to mention many others.

TABLE 1

(FIGURES REPRESENT PERCENTAGES OF JUDGMENTS OF SHORTER, EQUAL AND LONGER IN TERMS OF THE SECOND SPATIAL INTERVAL COMPARED WITH THE FIRST)

Time Intervals: 300 and 300 sigmæ

Observers	C			H		
Space	S	E	L	S	E	L
40–50			100			100
40–45		40	60			100
40–40	20	50	30	20	60	20
45–40	70	30		30	50	20
50–40	100			90	10	

We present, first, in Table 1, the case where no *Tau* effect is present or to be expected, t_1 equal to t_2. This table shows that when there is no temporal difference in the time intervals the majority of judgments approximate the actual facts: Sj_2 varies directly with the changes in s_2. Even when s_2 and s_1 are equal, we do not get 100 percent of E judgments, or 100 percent of L judgments when s_2 is longer than s_1. The judgments scatter according to the probability function, the majority approximating the actual distances. From Table 1 we are justified in saying that if the intensity, temporal intervals and all other factors except the spatial intervals are equalized, judgments of space tend to follow the actual distances between stimuli, or, in summary fashion:

If $t_2 = t_1$ and $s_2 = s_1$, then $Sj_2 = Sj_1$. (Case 1.)
If $t_2 = t_1$ and $s_2 > s_1$, then $Sj_2 > Sj_1$. (Case 1*a*.)
If $t_2 = t_1$ and $s_2 < s_1$, then $Sj_2 < Sj_1$. (Case 1*b*.)

Cases 1*a* and 1*b* are, like all discriminible functions, subject to the Weber-Fechner law, for even though s_2 is objectively greater or less than s_1 we do not expect a noticeable difference until the objective difference has reached a certain critical value. We must regard these two cases as subject to this restriction. On the other hand the importance of these cases is largely negative so far as the *Tau* effect is concerned

since they merely show that when the temporal intervals are equal, judgments of space are what we expect. They also serve to show that the spatial changes following changes in time are really due to the latter because, all other factors kept constant, when the time intervals are equal no spatial distortions appear.

But as soon as we make t_2 different from t_1, then the majority of judgments, instead of conforming to the actual relations obtaining between s_2 and s_1, conform rather to the relations between t_2 and t_1. In cases where the Os attempted to isolate the spatial factor, attempting to compensate for the *Tau* effect, by changing the spatio-temporal relations often enough it was possible to baffle them completely so that they could not tell whether they were 'coming or going' and found it easier to adopt the attitude called for in the instructions.

Tables 2–6 show the distribution of judgments when t_2 is *less* than t_1. The second spatial interval is lengthened in 5 mm steps until all the judgments are greater. In table 2 the difference between t_2 and t_1 is greatest, bearing a ratio of 200 : 500. In the following tables t_2 is increased by 50 sigmæ until, in table 6, the ratio is 400 : 500. It will be seen that the more nearly equal t_2 and t_1 are, the sooner do the judged spatial intervals approximate the actual distances. For the

TABLE 2

TIME INTERVALS: 500 AND 200 SIGMÆ

Observers	B			C			H			N			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30mm.	100			100			100			100			100			100		
30–35....	100			95	5		100			100			100			100		
30–40....	5	25	70	70	25	5	70	20	10	100			100			100		
30–45....	50	25	25	40	20	40	45	35	20	75	25		50	20	30	85	15	
30–50....		75	25	25	20	55	15	10	75	5	75	20	45	25	30	10	25	65
30–55....		30	70	25	10	65	15	30	55	30	45	25	15	15	70	40	15	45
30–60....			100	70	20	10	15	30	55	10	65	25	10	10	80	25	10	65
30–65....			100	10	20	70			100		10	90	20	20	60	5		95
30–70....				15	35	50					25	75	10		90	25	5	70
30–75....				5	35	60						100			100			100
30–80....						100												

TABLE 3

TIME INTERVALS: 500 AND 250 SIGMÆ

Observers	B			C			H			N			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30 mm.		15	85	100			100			100			100			100		
30–35.....	10	75	15	100			100			40	40	20	50	50		100		
30–40.....	5	60	30	90	10		40	30	30		80	20	60	20	20	70	10	20
30–45.....	10	60	30	70	20	10	40	10	50	10	50	40	30	60	10	40	40	20
30–50.....	20	55	25	75	25		20	30	50	10	10	80		30	70	40	10	50
30–55.....		10	90	25	35	40		20	80		30	70	20	30	50	10	30	60
30–60.....			100	25	25	50			100	20	60	20		10	90	10	20	70
30–65.....				10	35	65				10		90		10	90	60	20	20
30–70.....						100						100			100			100

TABLE 4

TIME INTERVALS: 500 AND 300 SIGMÆ

Observers	B			C			H			N			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30 mm...	90	10		100			95	5		100			75	20	5	65	10	25
30–35.....	100			85	5	10	35	35	30	40	40	20	45	10	45	15	15	70
30–40.....	75		25	50	15	35	25	25	50	45	35	20		10	90	15	15	70
30–45.....	45	30	25	30	10	60	35	35	30	10	45	45		5	95	20	15	65
30–50.....		10	90	5	10	85	20	15	65		25	75			100			100
30–55.....			100	5	5	90			100		10	90						
30–60.....						100					30	70						
30–65.....												100						

TABLE 5

TIME INTERVALS: 500 AND 350 SIGMÆ

Observers	B			C			H			N			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30 mm.....	85	10	5	90	10		75	25		80	20		40	25	35	75	15	10
30–35.....	10	80	10	90	10		30	60	10	10	90		20	30	50	55	20	25
30–40.....	10	20	70		50	50	10	80	10		50	50		10	90		5	95
30–45.....		5	95		30	70		10	90		10	90		10	90			100
30–50.....			100			100			100			100			100			

TABLE 6

TIME INTERVALS: 500 AND 400 SIGMÆ

Observers	B			C			H			N			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30 mm.	95		5	85	5	10	55	40	5	70	30		85	15		60	25	15
30–35.		30	70	60	15	25	15	45	40		25	75	10	25	65	35	30	35
30–40.	20	20	60	25	20	55	5	20	75			100		10	90	10	20	70
30–45.			100		5	95			100						100	25	5	70
30–50.						100												100

sake of brevity we present only one table of results showing that we may reverse the *Tau* effect by making t_2 *greater* than t_1 whereupon we must shorten s_2 (or lengthen s_1) in order for Sj_2 to approximate the actual spatial extents. This is shown in Table 7.

TABLE 7

TIME INTERVALS: 250 AND 500 SIGMÆ

Observers	B			N			H			C			Lew.			W		
Space	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L	S	E	L
30–30 mm.		10	90			100			100		10	90			100			100
35–30. . . .		30	70			100			100			100			100	30	20	50
40–30. . . .		60	40			100			100			100		20	80	20	30	50
45–30. . . .	50	50				100	30	10	60		40	60		10	90	50	50	
50–30. . . .	60	30	10			100		20	80		20	80	30	30	40	80	20	
55–30. . . .	60	40		20	70	10	30	20	50		60	40		30	70	100		
60–30. . . .	10	70	20		30	70	20	20	60		60	40		20	80	80	20	
65–30. . . .	20	60	20		10	90	30	30	40	10	60	30	20	40	40	100		
70–30. . . .		10	90	70	20	10	60	20	20	60	40		20	40	40	100		
75–30. . . .	20	20	60		10	90	80	20		90	10		80	20				
80–30. . . .	80	20		20	60	20	100			100			85	15				
85–30. . . .	100			100									100					

The goodness or size of the *Tau* effect depends upon the size of the difference between t_2 and t_1, provided we have chosen proper spatial intervals. We may say that the greater the difference between the temporal intervals, the greater must the spatial difference in the opposite sense be in order to offset the *Tau* effect. Hence, with a large difference between t_2 and t_1 it is necessary to change s_2 greatly in order to obtain 100 percent 'correct' judgments of space. An examination of Tables 2–6 shows this to be true: in Table 2 where t_2 is less

than half as large as t_1, s_2 must be made over twice as great as s_1 before Sj_2 gives 100 percent L; but in Table 6, where t_2 is only four-fifths as large as t_1, s_2 has only to be made, on the average, one and one-half times as large as s_1 in order for Sj_2 to give 100 percent L. Stated in the form of a proportion these facts appear more clearly: when $t_1 : t_2 : : 5 : 2$, then in order for Sj_2 to be reported correctly 100 percent of the time, $s_1 : s_2 : : 2 : 5$. It happens, in this example, which summarizes our first set of experiments, that the ratio $s_1 : s_2$ is the exact reciprocal of the ratio $t_1 : t_2$. If the two ratios follow each other closely, then we have a true dependence of space on time. The following summary shows this to be true. (Absolute values of t_1 and t_2 are taken from Tables 2–6 directly; s_1 is standard; the value of s_2 is the average value at which all Os gave 100 percent L judgments showing the *Tau* effect was completely offset. The ratios are given in numbers that can easily be compared in Table 8.)

TABLE 8

t_1	t_2	s_2	s_1	$t_1 : t_2$	$s_2 : s_1$	t_1/t_2	s_2/s_1
500	200	75	30	10 : 4	10 : 4	2.50	2.50
500	250	65	30	12 : 6	13 : 6	2.00	2.17
500	300	55	30	10 : 6	11 : 6	1.66	1.83
500	350	50	30	10 : 7	10 : 6	1.43	1.66
500	400	45	30	10 : 8	9 : 6	1.25	1.50

It is easily seen that when the time difference between t_1 and t_2 is not great, then the *Tau* effect disappears quickly as shown by the small ratio of s_2 to s_1. We must also notice that when conditions are not so favorable to the *Tau* effect the temporal and spatial ratios depart more and more from each other, as shown by looking down the last two columns of Table 8 above.[7] This is probably due to the fact that

[7] When the ratios between s_2 and s_1 and t_2 and t_1 are not about equal it does not necessarily mean that conditions are unfavorable to the *Tau* effect. It is conceivable that the ratios might be different yet the presence of the *Tau* effect in no wise impaired. Our present results show a fair constancy in ratio with the most favorable conditions for the *Tau* effect. If, for some reason, conditions were extraordinarily favorable to the *Tau* effect then the ratios might still be quite different. The imbalance in ratios tells us that the symmetrical relationship has been disturbed by exceedingly favorable or unfavorable factors.

as the *Tau* effect becomes weaker the factors responsible for 'correct' judgments intrude more and more, disturbing the dependence of space on time so that the time difference alone no longer forces a space difference with a consequent divergence in their respective ratios. But the close agreement in ratios shows beautifully the dependence of Sj_2 upon the temporal intervals and enables us to draw the following conclusions from our results:

If $t_2 < t_1$ and $s_2 = s_1$, then $Sj_2 < Sj_1$. (Case 2, Tables 2–6.)
If $t_2 > t_1$ and $s_2 = s_1$, then $Sj_2 > Sj_1$. (Case 3, Table 7.)

In Case 3 we have reversed the effect by making t_2 greater than t_1, showing that it does not matter whether the second temporal interval is longer or shorter than the first so far as its influence upon the experienced spatial interval goes.

In the above cases we have drawn our conclusions on the supposition that the spatial intervals are equal. If a longer spatial interval can be made to appear shorter by decreasing the temporal interval, then we know that the *Tau* effect is indeed compelling. Inspection of the tables shows this to be true so that we may summarize as follows, understanding the brackets to mean 'within limits':

If $[t_2 < t_1$ and $s_2 > s_1]$, then $[Sj_2 \leqq Sj_1]$. (Case 4.)

The reverse effect may be stated:

If $[t_2 > t_1$ and $s_2 < s_1]$, then $[Sj_2 \geqq Sj_1]$. (Case 5.)

Cases 4 and 5 show us that even though s_2 may be longer (shorter) than s_1, it may be made to appear shorter (longer) by shortening (lengthening) t_2 sufficiently. The greater the difference between s_2 and s_1, the greater will the difference between t_2 and t_1 have to be for these laws to hold, hence the brackets around Sj_2 which tell us that a distance may be chosen for s_2 which will be so large (small) as compared with s_1 that no temporal interval, t_2, no matter how short (long), will suffice to produce the *Tau* effect. These last two cases show that not only is it possible to make one of two equal spatial extents longer or shorter than the other by varying the

time intervals between stimulations defining the extents, but also that, within certain limits, the shorter or longer of two extents may be made to appear longer or shorter than the other if the temporal interval between stimulations defining it be properly chosen. In short, our last four cases (1–2 inclusive) enable us to say that judged spatial extents defined by successive stimulations depend more upon the temporal intervals and their relations to each other than upon the actual distances between stimuli.

Stimulation of Only Two Points Defining Two Spatial Intervals and the Problem of Local Sign.—Instead of using three different spots on the skin which was done in the experiments described above, it is possible to define two spatial extents by successive stimulation by means of only two spots if the first spot stimulated is also the third. The order will then be 1–2–1. The question arises: Will the *Tau* effect appear if the two extents are identical and identical spots are used for the second interval? If it does, then the doctrine of local sign, according to which every spot on the skin possesses its own peculiar spatial *quale* whereby it is spatially anchored, must fall. In Tables 9 and 10 the results of two Os are given when only two spots were used to define the spatial extents.

TABLE 9

TIME INTERVALS 1,000 AND 400 SIGMÆ

Obs.	B			H		
Space	S	E	L	S	E	L
30 mm.....	90	10	0	75	25	0
80 mm.....	95	5	0	10	90	0

TABLE 10

TIME INTERVALS 250 AND 100 SIGMÆ

Obs.	B			H		
Space	S	E	L	S	E	L
30 mm.....	70	5	25	35	65	0
80 mm.....	70	5	25	20	75	5

While the above tables show that the *Tau* effect is more variable when the third spot of stimulation is identical with the first, nevertheless the results are sufficiently good to substantiate the statement that if the temporal interval between 2–1 is different from that between 1–2, the distance appears changed even though all factors except the temporal are identical. Indeed, spot 3 often seemed to be in a different position and to have an altered quality under these conditions. An added phenomenon appeared when the Os reported that four spots were stimulated, the third and fourth being felt to the side of 1 and 2. And even when Sj_2 was reported equal to Sj_1, the impression often persisted that the places stimulated were different for the second interval from the first.

Three spots were then selected on the arm arranged in the form of an equilateral triangle, the points being 40 mm apart. When the time intervals between stimulations were equal a fairly symmetrical, recognizable figure was sensed. But when the time intervals were 300 and 200 sigmæ between the stimulations, a distorted figure was perceived.

While much more remains to be done along these lines, it is evident that spots on the skin do not possess a spatio-qualitative characteristic independent of each other or the mode of stimulation. In these experiments identical spots did not serve to establish identical spatial patterns when the temporal factor was changed. Direction, distance, localization and spatial structures on the skin are therefore determined by a complex of factors in which the so-called local sign is not a constant from which the former are derived. It is only under rather constant conditions making for firm structures that individual spots within the pattern exhibit fixed spatial references. As soon as the conditions become more labile, *e.g.*, with short temporal intervals, fixed local sign disappears. A satisfactory theory of space perception must account for all the facts, and with our findings we find the classical theory at variance. What the primary datum is in space perception, if it is not space itself, must await further experimentation, but certainly the assumption of a local sign for each spot on the body with the implication that space perception consists of the compounding of many of these is false.

Disintegration of the Tau Effect.—There are several ways in which the *Tau* effect may be made to disappear or fail to appear. Far from showing that it is a lightly dispelled illusion, its absence rather requires special conditions, either on the part of E or O. We may mention first that the *Tau* effect will not appear if, taking the second spatial interval much longer or shorter than the first, we do not change the temporal interval between stimulations sufficiently to overcome the difference. On the other hand, if the two spatial distances are equal, the temporal difference must still be sufficiently great to produce the effect in the majority of observations. Tables 9 and 10 indicate that we were working under conditions less favorable for the *Tau* effect when the spatial extents were very large (80 mm) and when both temporal intervals were comparatively short (250 and 100 sigmæ). When the extents are large, a large change is necessary in order to be observed. Likewise when the time intervals are very short it is more difficult to perceive the effect they undoubtedly exert.

Finally we must mention as important for the appearance of the *Tau* effect the attitude of the O and whether or not he follows instructions. If the O complies with instructions to judge quickly and naturally the experienced spatial intervals, we may say that 'subjective' factors favor the *Tau* effect. But if at the time of observing and in his later ratiocination the O attempts to isolate the spatial from the temporal aspect his results may be more often 'objectively' correct but less consistent than those of Os who follow instructions and get the effect. All in all, however, the *Tau* effect is so easy to demonstrate that it may be performed with any subject as a parlor trick or game.

Theoretical Considerations.—The effect of time upon a large variety of experiences and modes of behavior is still *terra incognita* from the laboratory point of view.[8] Its effect upon attention, span of apprehension, the feelings, learning

[8] H. Ebbinghaus discusses the problem of the experience of time. He has found the indifference time, *i.e.*, that temporal interval which is judged correctly most often by being neither over- nor under-estimated to be .6 to .7 second. See his *Grundzüge d. Psychologie*, 1902, vol. 1, 465. See H. Woodrow, the reproduction of temporal intervals, this *Journal*, 1930, 13, 473 ff.

and fatigue curves and a host of other phenomena have long been known. But usually the temporal factor is regarded as subsidiary to others. Since in our experiments all other factors have been kept constant, or relatively so, we must attribute our findings to time and time primarily. The temporal factor must enter as an integral part of the causal complex determining the experience of space. We must therefore regard the neurological processes underlying the experienced spatial intervals as capable of profound modification through the influence of the temporal factor. Just as an enclosed space in the Müller-Lyer figure and its many variants appears smaller than an open interval, so a space defined by a short interval of time appears smaller than one defined by a long interval. We assume that the physiological processes aroused by stimuli following each other at a brief interval become more intimately bound together than when the temporal interval is long. In our experiments it appears as if a separation in time is accompanied by a separation in space of those processes underlying the felt local stimulations. Whether different spots in the brain are stimulated when the same places on the surface are stimulated at different temporal intervals, we do not venture to say. The fact that when the times are sufficiently short not two spots are felt but one moving impression shows that the underlying physiological processes have, so to speak, become as good a unit as possible for in this case there is no spatial separation and the interval traversed is a minimum as pointed out above.

It is conceivable that the time of arrival of the waves of excitation from each spot at or through some common region in the brain may be responsible for the *Tau* effect. In this case we find that the time *between* stimulations is of supreme importance just as in the usual phenomena of chronaxy the time *of* stimulation is of importance. We might call the time factor here the 'inter-chronaxy' to distinguish it from the time of stimulation of a single spot which now bears the label of chronaxy. It is known that differences in chronaxy alone, the rheobase remaining constant, are sufficient to evoke differences in sensation. Differences in 'inter-chronaxy'

might then be expected to be followed by felt differences in the intervals between stimulations. This fact was recognized as long ago as 1901 by Kramer and Moskiewicz and 1906 by E. Janesch [9] when they found that the judgment of the extent of an arm movement depended *inter alia* on the time it takes to carry out the movement. But its importance could not be realized until the work of Lapicque [10] demonstrated that the time factor as such is an integral part of many physiological processes.

With the recognition by physiologists of the part played by time in physiological processes it is hardly necessary to resort to a purely psychological theory involving 'error of judgment' (*Urteilstäuschung*-Benussi) to account for the *Tau* effect. Gelb, in a brief space, has adequately dealt with such theories.[11] Rather, we must assume that the temporal intervals between stimulations have a direct physiological effect on the central fields mediating the experienced stimulations and the spatial intervals between them. The *Tau* effect then becomes a special case of that large class of phenomena ranging all the way from the simple muscular twitch to memory and learning which are profoundly affected by the temporal factor. At least this point of view, we believe, enables us to correlate the *Tau* effect with many other better known phenomena and is suggestive of further experimentation.

[9] F. Kramer and G. Moskiewicz, *Z. f. Psych.* Beiträge zur Lehre von den Lage- und Bewegungsempfindungen, 1901, 25, 101–125; E. Jænsch, Ueber d. Beziehungen von Zeitschätzung und Bewegungsempfindungen, *ibid.*, 1906, 41, 269ff.

[10] L. Lapicque, *L'excitabilité en fonction du temps;* la chronaxie sa signification et sa mesure, 1926.

[11] *Op. cit.*

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THE EFFECTS OF CHANGE OF WORK ON THE WORK DECREMENT

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"In my own case I have been interested in observing how every prolonged period of monotonous work like correcting papers, for example, finds before its close some insistent demand for interruption. If I successfully suppress one demand, more insistent ones arise, until finally effective voluntary reinforcement of the main task suddenly ends." This has been the experience of Dodge (1917, p. 111) and countless others when it has been necessary to continue activity at one task for a period of time. Industry, educational science, and personal interests are all concerned with demands to vary the work program. Slogans such as, "Variety is the spice of life," and" Nothing kills like monotony," bear testimony of the interest and evident belief of the lay mind in the effectiveness of a program of work that will allow for sufficient variety. The high type of specialization of activity now in demand in our complicated industrial centers has led to numerous discussions of the need for recreation of such workers, for change in activity. The British Industrial Fatigue Research Board has made some study of change of work in its relation to fatigue problems of industry. We are still largely ignorant of the nature of the work decrement due to continuous periods of mental work and of the means of reducing such a decrement when it does occur. This study is an attempt to make some slight contribution toward the solution of one phase of this difficult problem, namely, the effects of change of work on the work decrement due to mental activity.

The Problem

The problem of the work decrement is an old one in experimental psychology, and explanations of its cause are nu-

merous. The earliest and most commonly ascribed cause has been that of mental fatigue. Thorndike (1914) and Dodge (1917) both oppose this explanation. Dodge (1917) in an address before the American Psychological Association states that, "fatigue has served as a class name of unanalyzed hinderances to effective work" (p. 89). In regard to the effects of change of work on the work decrement he says: "Moreover, there are no facts available to show that restriction to a single field like multiplication will produce greater work decrement than rapid change from one field to another. On the contrary there is evidence that the greater the complexity of the mental task the more pronounced is the decrement. . . . Such decrement, however, is more probably due to a confusion between paths of discharge than to fatigue of any particular path" (p. 97).

Thorndike (1914) has questioned the existence of a decrement resulting from continuous work. He goes on to say that, "The most important fact about the curve of efficiency of a function under two hours or less continuous maximal exercise is that it is, when freed from daily eccentricities, so near a straight line and so near a horizontal line" (1914, p. 69). If any decrement appears he attributes it to lack of 'satisfyingness' resulting from monotony of occupation. Such a conception does not lend itself to objective criticism.

Most studies of the work curve confirm the irregularity of the decrement for varying types of work. This very fact seems to have resulted in extreme caution on the part of experimental psychologists. Few studies have appeared since the critical analysis of the problem by Dodge.

On the theoretical side Dodge has formulated two laws of fatigue. They are: "Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the stimulus" (1917, p. 102). "In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from competition in favor of the less fatigued tendencies" (1917, p. 105). Robinson (1926) has enumerated six principles of the work decrement. He states that the decrement is relative to the recency and

frequency of the functioning of the connection, to the relationship of the stimulus with other responses, to the strength of the specific connection, to the quantative constancy and qualitative integrity of the stimulus, and to other decrements which have developed in other stimulus-response connections.

Since so many conditions, such as, variations in external stimuli and in internal bodily conditions, may affect the development of the decrement, and since these conditions have not yet been subjected to experimental analysis, this study will not attempt an analysis of the causes of the decrement; but will make a study of the decrement as it exists under as well controlled conditions in certain conditions as are possible. The investigation will attempt to find out whether or not under certain given circumstances change of work will increase, decrease, or eliminate the decrement incurred by continuous work. Change of work is interpreted to mean change from one type of mental activity to another.

History of the Problem

Since all early studies in problems related to the work decrement accepted fatigue as the cause of the phenomenon, a complete history of the problem would begin with an experiment by the Russian psychiatrist, Sikorski, in 1879. Mosso (1894), Binet (1898), Offner (1911), and Thorndike (1914) have reviewed the earlier studies, hence, it will not be necessary to repeat reviews of them here.

One of the greatest difficulties encountered by the early experimentalist in their studies of the work decrement was the exclusion of practice gain. Kræpelin (Seashore, 1903) recognized that practice effects might over-shadow a genuine fatigue up to a certain point. But, in reality he interprets fatigue as the decrement which begins when practice gain is no longer able to over-shadow the fatigue due to continuous work, and when the work curve declines rather than rises. One of the early attempts to control practice effects was made by Thorndike in 1900. He did not give the same multiplication problem twice to the same group. This method did not eliminate from practice the numerous combinations which

were bound to re-occur in different problems. For instance, 385 × 225 and 453 × 158 are not identical, yet both involve the products of 5 and 5, of 5 and 8, and of 3 and 5. Another method has been to work out all practice gain before the period of testing begins. Up to the present time this method seems best. Marsh (1906) eliminated his practice gain mathematically. Laird (1925) attempted to control practice effects by an elaborate rotation of groups.

Two methods have been used to equate groups. The one most commonly employed in recent studies is division on the basis of scores on an intelligence test. The older method was to equate groups on the basis of an initial score on the test used as a work test. This method assumed that practice gains would remain proportionate in the groups so divided. Hollingworth (1914) has shown that such a method does not insure equal future gain.

Few studies have used simple or comparable stimuli. With regard to types of stimuli Offner (1911, p. 97) states that change of work has the effect of a rest when the change is to less difficult work than that in which the decrement has developed. However, we have not as yet a method for determining the relative difficulty of stimuli. Robinson and Bills (1926) have made some investigations of the relationship of varying degrees of homo-heterogeneity of activities to the work curve.

Most of the earlier studies employed too few subjects, and individual differences were so pronounced that conclusions based on them are unwarranted. Where groups were used, the reliability of the differences of the groups was not stated, and conclusions were drawn from small differences. Sometimes the subjects were the experimenters themselves. Often the subjects were aware of the purpose of the experiment. The age factor was not very carefully controlled in group work, some experiments including children and adults together.

Chapman (1916), Morgan (1926), Phillips (1920), and Poffenberger and Tallman (1915) have investigated the development of the decrement in the mental work curve. Phillips found, when he used units of one minute that there

was high initial performance the first minute, and that there followed a marked depression the second minute of work. This depression was followed by a rise the third minute and then a continual depression. Poffenberger and Tallman found, by using half minute periods, that the second half minute showed the greatest decrement. Morgan concluded that the second period of a short working unit shows depression rather than the second minute as shown by both Phillips and Chapman.

The Experiment

The tests used in this experiment were the Peterson Uniform Test of Intelligence developed by J. C. Peterson (1928) and the 'A' Cancellation Test (Whipple, 1910). The Peterson Intelligence Test is an equation test. It is made up of two pages of equations like the following: 5 4 2 = 3, 7 = 2 5 3, 8 2 = 3 1. The subject is to make each series of four numbers a true equation by inserting at the proper points the necessary signs of addition, subtraction, multiplication, or division. The equation test is furnished in two forms, A and B. The writer arranged a modification of the 'A' test in order to have enough A's on one page for five minutes of practice. Four different forms were made, each of the last three being a changed arrangement of the first form. The number of A's to the page were practically the same on all four forms. The tests were checked for their reliability after three weeks of preliminary practice. The reliability of the equation test at the time was .87 while that of the cancellation test was .77. The author of the equation test has measured the reliability of that test after ten minutes of practice. He finds it is .856. When the reliability coefficient is based on two 30 minute periods of work without a preliminary practice, it is .938.

The subjects used in the experiment were young women enrolled in the psychology department at Alabama College.[1]

[1] The writer wishes to thank Dr. T. H. Napier and Dr. Marie Hækel Means for their coöperation and for the use of members of their psychology classes as subjects in this experiment; also Dr. T. L. Kelley for valuable suggestions in the statistical treatment of the data. The writer is greatly indebted to Dr. Joseph Peterson for his constant encouragement and helpful criticisms during the course of this study.

Since practically all the subjects were boarding in the dormitory where hours for sleeping and eating were regular, conditions seemed unusually well controlled for the group. The subjects were asked to refrain from eating at irregular hours during the week of the experiment and also to observe regular hours for sleep.

Classes at Alabama College meet three times per week, on Monday, Wednesday, and Friday or on the intervening days. For the preliminary practice periods it was not practical to attempt to get the entire group together except on days when their classes were in regular session. On January 7, 1929, the practice tests were begun. All classes were tested at their regular class period. All subjects practiced on both the equation test and the cancellation test. Students were to practice in their rooms on days when their classes were not in session. Since the tests made in the rooms were likely to be irregularly timed, the results of the practice periods were not tabulated. The purpose of the preliminary practice periods was to get practice effects worked out of the subjects before they began the experiment. A sampling of the group showed the practice gain to be between one and two percent by January 28th. The subjects were then divided into groups. The Otis Self-Administering Test of Intelligence, Form A, was used as basis for equating the groups. Correlation between the Otis and the equation test before the three weeks of preliminary practice was .51, with the cancellation test the correlation of Otis was .34. These coefficients were based on 205 cases each. The range of scores on the Otis S. A. was from 30 through 69. The mean of the group was 47.75 with sigma of 9.15. The subjects were divided into four groups of 45 each and designated as Group I, II, III, and IV. After the division had been made on the basis of score on the intelligence test, the groups were checked to determine the number of freshmen, sophomores, juniors, and seniors in each group. Some redistributions were made in order to equate the number from each class in the several groups as nearly as was possible. The groups were also checked with regard to the ages of the subjects and some shifts were made

to equalize this factor. Ages of the subjects ranged from 15 through 23. The mean age was 20.3 with sigma 1.4.

During the final week of testing all subjects met at 4:30 P.M. The groups worked for 20 minutes. The 20 minute work period was divided into four intervals of 5 minutes each. Throughout the study these intervals will be designated by Arabic numerals, while the groups will be designated by Roman numerals.

Table 1 shows the plan of work for the four groups. Each interval represents 5 minutes of work. C stands for the cancellation test and E for the equation test.

TABLE 1

Interval	Group I	Group II	Group III	Group IV
1	E	E	C	C
2	E	C	E	C
3	E	E	C	C
4	E	C	E	C

Groups I and IV worked continuously at the equation and cancellation tests, respectively, while Groups II and III changed work according to the scheme shown in Table 1. At the end of each five minute interval the experimenter called 'Time.' Group I merely marked the point reached and continued work. The other three groups shifted papers. The amount of time required to shift pages seemed negligible, but in case there was an advantage in not having to shift, the advantage is in favor of Group I.

The testing began on Monday, January 28th, and continued through Friday, February 1st. It was not thought advisable to attempt to hold the groups to work on Saturday afternoon, as so many were away for the week-end.

The Results

The experiment began with 205 subjects. At the end of the three weeks of preliminary practice there were 180 subjects assigned to the four groups. Of the 180 subjects only 89 attended all the work periods for the final week of testing.

The large loss of subjects was due to the 'flu' epidemic then in progress over the state. The 180 subjects had been quite evenly divided into four groups on basis of their intelligence scores, but the dropping out of so many upset the equality of the grouping. In the original division when a subject making a score of 50 was placed in one group, another similar individual was placed in each of the other groups. When it was necessary to place an individual making a score of 51 in one of the groups and the other three groups contained individuals making scores of 50 each, the next time the group receiving the individual with a score of 51 received an individual with a lower score. The total score of the several groups was the same. Such an arrangement was not possible with the 89 remaining subjects. The groups were re-equated so that each group contained 20 subjects. The nine subjects who were dropped were left out because their intelligence scores did not fit well into the scheme for re-equating the groups. After the best possible arrangement had been made the means and the sigmas of the four groups were:

TABLE 2

Group	Mean	Sigma
I	48	9.35
II	48.5	9.95
III	48	8.65
IV	47.5	9.1

The correlation of the Otis S. A. and the equation test before the preliminary practice began was .51. After three weeks of practice the correlation was .45. The correlation of the Otis S. A. and the cancellation test before the preliminary practice was .34. After three weeks of practice it was .25. This is as much correlation as would be expected according to reports summarized by Whipple (1910) relative to the relationship between the cancellation test and tests of intelligence.

The tests were scored for number right and for errors. The reliability of the right scores on the equation test was .87. This reliability coefficient was obtained by correlating the results in terms of right scores in the first interval on

Monday with the results in terms of right scores in the first interval on Tuesday. This coefficient would seem to be fairly representative of the reliability of the test at other intervals and for other days since the coefficient of variability for the successive days of the week ran .30, .33, .30, .33, .36. The reliability of the cancellation test was obtained in like manner and was .77. The coefficients of variability for this test on successive days of the week ran .13,. 14, .14, .14, .13. Kincaird (1925) has summarized a number of studies relative to variability of groups during practice. She states that while differences generally decrease with practice, the relative standing of the individuals tends to remain the same. Hence, the reliabilities given above are probably closely representative of the total reliability for the groups.

At the stage when the subjects were divided into groups for the final tests, practice effects were almost nil. However, the change in work conditions brought about a rise in practice improvement. Since the subjects did not know the purpose of the experiment, they concluded that the four groups were being tested for superiority and a great deal of rivalry developed. The three groups working on the equation test showed the following gains over their Monday score:

TABLE 3

EQUATION TEST

Per cent Gain Over the Score for Monday Shown According to the Per cent Increase Each Day

	Group I	Group II	Group III
Tuesday	17.05%	4.35%	5.49%
Wednesday	26.14%	19.56%	17.58%
Thursday	41.48%	20.65%	28.57%
Friday	42.04%	27.17%	25.27%

The practice gain in Table 3 was figured as the per cent of improvement of the mean of the total scores of each successive day over the mean of the total score for Monday. The per cent gain was not figured for the various intervals of the day, but from the total score for the day.

The similar gains in the cancellation test made by the three groups working on this test were:

TABLE 4

CANCELLATION TEST

Per cent Gain Over the Score for Monday Shown According to the Per cent Increase Each Day

	Group IV	Group III	Group II
Tuesday	5.78%	6.79%	8.38%
Wednesday	3.70%	7.08%	6.88%
Thursday	12.96%	10.76%	7.19%
Friday	17.36%	7.36%	11.97%

The practice improvement was then calculated for the individual scores according to the following plan. The per cent of practice gain in Group I for Tuesday is 17.05. Each subject's score for each interval was accordingly divided by 117.05. Such a scheme worked an injustice to some individual scores. Some scores were lowered too much and other scores did not get all the practice effect worked out. However, since the per cent of improvement of the group would have less error than any individual per cent of improvement, this plan was thought the best one. Another plan, however, was tried in order to compare results. The individual scores were computed in a similar manner to the computation for the group. The per cent gain of each individual was computed in terms of his gain over his score for Monday. The results of this method were not very different from those of the first method. However, the reliability of an average is always greater than the reliability of any single score. For this reason the results of the first method are reported.

Practice improvement on the equation test and the cancellation test after three weeks of practice cannot be predicted by the Otis S.A. The correlation between the Otis S.A. and the average per cent of improvement of each individual during the last week of testing was .02 for the equation test and .03 for the cancellation test. Atkinson (1929) used the card-sorting and substitution test. He found that intelligence

tests did not predict limits of ability as well as did tests of mechanical speed when such tests were well chosen. He stated that intelligence and learning ability as measured by the two tests he used were not identical.

All scores used in the final results are number right scores. Error scores varied so widely for different days and different intervals that it seemed wise to check the nature of their variability. The correlation of right scores with lack of errors on the equation test was .49; while it was −.19 for the cancellation test. The relationship of the errors to the right scores on the two tests were different. There was a decided tendency for high scores to have few or no errors on the equation test, and there was a slight tendency for errors and scores to vary together in the same direction on the cancellation test. Errors were then computed as per cent of the right score. Correlation on the equation test of the right scores with lack of errors, when the error score was expressed as per cent of the right score, was .47; while on the cancellation test it was .06. Since error scores were so variable from individual to individual and from day to day, a check was made of their reliability. Reliability of error scores on the equation test was .35, and on the cancellation test it was .31. Since reliability was so low, the error scores were not considered in the final results.

It was the original plan to measure the effects of work on the five days of the week and to determine what variability was due to the day of the week. But, since practice effect showed up in the results and had to be figured out, this became impossible. The results of practice gain show that a rapid improvement takes place from Monday to Tuesday, but this may be in a manner comparable to the rapid gain in early stages of learning rather than to the influence of the day of the week on the work curve. After practice effects had been eliminated for all five days of the week, the results for the five days were lumped. In the results given below in Table 5 the score for Group I, Interval 1, of the equation test is the mean of 100 scores made by the 20 individuals in the group working for five days. The same is true of the scores for other groups and intervals.

TABLE 5

EQUATION TEST

Means and Sigmas of 20 Individuals Working 5 Days (100 Scores) on the Equation Test

Group I worked continuously at the equation test. Groups II and III changed work each interval of five minutes.

Interval	Continuous Work			Change of Work		
	Mean	Sigma	Group	Mean	Sigma	Group
1	60.05	16.99	I	59.90	21.45	II
2	53.50	15.38	I	58.30	17.70	III
3	53.20	17.11	I	56.80	21.90	II
4	52.95	18.04	I	56.00	16.91	III

Differences of the Means of Groups

Interval	Difference of Means	Sigma of Difference*	Critical Ratio	Chances of a True Difference Greater than Zero
1	Difference negligible			
2	4.8	1.75	2.73	99.7 in 100
3	3.6	2.47	1.45	93 in 100
4	3.05	1.77	1.71	96 in 100

* The obtained r's for the intervals in Table 5 were as follows: Interval 2, $r = .446$; Interval 3, $r = .215$; Interval 4, $r = .483$.

In Tables 5, 7, 8, and 10 the sigmas of the differences of the means were figured by the long formula given by Yule (1919), $\sigma^{\circ}_{diff} = \sqrt{\sigma_1^2 + \sigma_2^2 - 2r\sigma_1\sigma_2}$. The r used was the result of the correlation of the scores on Otis of corresponding individuals within the equated groups. In other words, the individual whose score on the Otis S.A. was highest in Group I was designated as individual 1 for that group, the second highest as 2, and so on. The same procedure held for all other groups. It was assumed that individual 1 in Group I was the equal of individual 1 in Group II, etc, and correlations were made accordingly.

For a discussion of the use of the long formula with equated groups the reader is referred to an article by Walker (1929).

Taking into consideration the reliability of the test when computing the sigma of the difference as suggested by Tryon (1926) the results are:

TABLE 6

EQUATION TEST

Differences of Means of Groups and Critical Ratio Calculated from Tryon's Formula

Interval	Difference of Means	Sigma of Difference	Critical Ratio	Chances of a True Difference Greater than Zero
1	Difference negligible			
2	4.8	2.18	2.25	99.7 in 100
3	3.6	2.59	1.19	88 in 100
4	3.05	2.30	1.32	90 in 100

Tryon's formula is: C.R. $= \frac{nd^2}{\sigma_a^2(r_a) + \sigma_b^2(r_b)}$; r in this instance was .87.

The results given in Tables 5 and 6 indicate not only the effects of change of work but also those of continuous work. It is evident that at the end of Interval 2 practice is not equal for Groups I and III. Group I has worked ten minutes on the equation test while Group III has worked only five minutes on the equation test and five minutes on the cancellation test. In order to make the groups comparable in regard to the amount of practice on the equation test and to isolate the value of change of work from the decrement due to continuous work it was necessary to equate the amount of time spent on the equation test.

The value then for one period of change of work for five minutes is the difference between the first interval for Group I and the second interval for Group III.[2] The value of change of work for the third interval is the difference between Group I at the second interval and that of Group II at the third interval. The amount of practice on the equation test is at this time ten minutes for both groups. Group II had changed work for five minutes. For the fourth interval, the value of change of work is the difference in the means at the end of the second interval of Group I and the fourth interval of Group III minus the value already determined for the effect of change of work in the second interval (the difference

[2] The writer is indebted to Dr. T. L. Kelley for the suggestion of this method of treatment of the data.

between Interval I of Group I and Interval 2 of Group III). The results of this method of treatment are given in Table 7.

TABLE 7

EQUATION TEST

Differences in Means of Groups Working Continuously and Those Changing Work When the Groups Are Equated with Regard to Amount of Practice

The method used to equate groups for practice is discussed in the text.

Interval	Difference of Means	Sigma of Difference	Critical Ratio	Chances of a True Difference Greater than Zero
1.....	Difference negligible			
2.....	−1.75	3.92	.445	67 in 100
3.....	3.3	2.25	1.466	93 in 100
4.....	4.25	1.62	2.62	99 in 100

The methods of obtaining the differences of the means and sigmas of the differences are summarized in Table 7*a*. The table should be read as follows: Difference of mean of Group I, Interval 1, minus mean of Group III, Interval 2, etc. The Roman numerals stand for the groups and the Arabic subscripts indicate the intervals.

TABLE 7*a*

METHOD OF OBTAINING DIFFERENCES OF MEANS SHOWN IN TABLE 7 AND THE FORMULÆ USED IN COMPUTING SIGMAS OF DIFFERENCE AS SHOWN IN TABLE 7

The long formula given by Yule (1919) is used to compute all sigmas of difference.

Interval	Difference of Means	Sigma of Difference*
2.....	$I_1 - III_2$	$\sqrt{\sigma_{I_1}^2 + \sigma_{III_2}^2 - 2r_{I_1III_2}\sigma_{I_1}\sigma_{III_2}}$
3.....	$I_2 - II_3$	$\sqrt{\sigma_{I_2}^2 + \sigma_{II_3}^2 - 2r_{I_2II_3}\sigma_{I_2}\sigma_{II_3}}$
4.....	$I_2 - III_4 - (I_1 - III_2)$	$\sqrt{\sigma_{I_2}^2 + \sigma_{III_4}^2 + \sigma_{I_1}^2 + \sigma_{III_2}^2 - 2r_{I_2III_4}\sigma_{I_2}\sigma_{III_4}}$

$$\sqrt{-2r_{I_2I_1}\sigma_{I_2}\sigma_{I_1} + 2r_{I_2III_2}\sigma_{I_2}\sigma_{III_2} + 2r_{III_4I_1}\sigma_{III_4}\sigma_{I_1} - 2r_{I_1III_2}\sigma_{I_1}\sigma_{III_2} - 2r_{III_4III_2}\sigma_{III_4}\sigma_{III_2}}$$

* Since those sigmas of the differences in the table above were also derived by the long formula, the reader is hereby given the r values:

Interval 2, $r = .348$; Interval 3, $r = .310$; Interval 4, $r_{I_2III_4} = .507$, $r_{I_2I_1} = .858$, $r_{I_2III_2} = .446$, $r_{III_4I_1} = .477$, $r_{I_1III_2} = .348$, $r_{III_4III_2} = .758$.

Results of the cancellation test have been figured in a similar manner. Tables 8, 9, and 10 are directly comparable as to the cancellation test to Tables 5, 6, and 7 obtained from

the equation test. Since the differences in Tables 5, 6, and 7 (with the exception of the second interval) are favorable to change of work, the differences favorable to continuous work are marked negative. This rule is applicable to all tables.

TABLE 8

CANCELLATION TEST

Means and Sigmas of 20 Individuals Working Five Days (100 Scores) on the Cancellation Test

Group IV worked continuously at the cancellation test. Groups II and III changed work each interval of five minutes.

Interval	Continuous Work			Change of Work		
	Mean	Sigma	Group	Mean	Sigma	Group
1	433.5	66.00	IV	438.85	56.59	III
2	430.5	65.27	IV	416.00	61.99	II
3	429.5	59.58	IV	438.25	60.67	III
4	432.25	65.35	IV	430.25	64.72	II

Interval	Difference of Means	Sigma of Difference*	Critical Ratio	Chances of a True Difference Greater than Zero
1	Difference negligible			
2	−14.5	3.91	3.70	100 in 100
3	8.75	8.33	1.04	85 in 100
4	− 2.00	9.14	.21	58 in 100

* The r's computed as in the equation test are: Interval 2, $r = .189$; Interval 3, $r = .039$; Interval 4, $r = .130$.

Sigmas of the differences were figured as were those on the equation test.

Using the formula of Tryon for computing sigma of the difference, the results are:

TABLE 9

CANCELLATION TEST

Differences of Means of Groups and Critical Ratio Calculated from Tryon's Formula

Interval	Difference of Means	Sigma of Difference*	Critical Ratio	Chances of a True Difference Greater than Zero
2	−14.5	7.89	1.84	80 in 100
3	8.75	7.45	1.17	87 in 100
4	− 2.00	8.08	.24	60 in 100

Segregating the effects of continuous work from the effects of change of work as was done for the equation test in Table 7 and the results are:

TABLE 10

CANCELLATION TEST

Differences of the Means of Groups Working Continuously and Those Changing Work When the Groups Are Equated with Regard to Amount of Practice

The method used to equate the groups for practice was discussed in connection with the equation test.

Interval	Difference of Means	Sigma of Difference†	Critical Ratio	Chances of a True Difference Greater than Zero
2.....	−17.5	7.92	2.09	98 in 100
3.....	7.75	8.68	.89	82 in 100
4.....	17.25	5.20	3.31	100 in 100

* In Table 9, $r = .77$

† The obtained r's (method shown in Table 7*a* for the equation test) are: Interval 2, $r = .234$; Interval 3, $r = .051$; Interval 4, $r_{IV_2II_4} = .152$, $r_{IV_2IV_1} = .809$, $r_{IV_2II_2} = .189$, $r_{II_4IV_1} = .006$, $r_{IV_1II_2} = .234$, $r_{II_4II_2} = .668$.

DISCUSSION OF RESULTS

A definite work decrement developed for the group that worked continuously at the equation test. The group working continuously at the cancellation did not show a noticeable decrement from continuous work. Change of work reduced the decrement on the equation test somewhat. The study made by Chapman (1917) for much shorter periods of work is in accord with these conclusions. He finds greater improvement due to the effects of change of work on addition than on cancellation. Rest is more effective on addition than is change to cancellation. From this fact he justifies the conclusion that the decrease in the product after the first half minute of work at addition is due to interference produced by previous work. He finds greater interference for addition than for cancellation. By interference he means in the case of addition that previously solved problems continue to retain their effects in the organism and to compete with the problem at hand. Dodge (1917) states a somewhat similar view. He regards the complexity of the task a contributory

factor in the work decrement. He says: "The greater the complexity of the mental task the more pronounced is the decrement" (1917, p. 97). Such decrement he considers due more to 'confusion between paths of discharge than to fatigue of any particular path' (1917, p. 97). This condition may account for the lack of decrement in the cancellation test and the development of decrement in the equation test. On the other hand, Robinson and Bills (1926) find in certain types of activity a greater decrement for simpler processes. They say: "Since relatively homogeneous work involves greater recency and frequency of functioning of each of its part-activities than does relatively heterogeneous work, we should expect psychological activities of a homogeneous sort to manifest more refractoriness and, therefore, more decrement than work of a more heterogeneous sort" (p. 442). Such a theory would anticipate a greater decrement in the cancellation test than in the equation test. This was not the case in this experiment. Robinson and Bills find in an experimental attack of this problem that the greater decrement is developed in more simple activities. They measured the amount of decrement incurred in writing successively two, three, and six letters of the alphabet. The greater decrement occurred in writing two letters, *ababab*, than in writing six letters, *abcdef*. In the latter phrase the writer must break his serial association. The authors recognized this possibility and tried out a number of other activities where they think there is need for 'more delicate adjustments' in heterogeneous material. They find the decrement for heterogeneous material equal to that for more homogeneous material in typing with one, two, and three fingers. They explain the apparently negative result in this instance by saying that the more heterogeneous task sets up competition between its part-activities which offsets the greater refractoriness of the more homogeneous work. For instance, there is more competition between part-activities involved in writing with three fingers than in writing with one finger. This competition offsets the greater refractoriness which results from continuously writing with one finger. Applying the same theory to the present problem, the greater

refractoriness set up by continuously cancelling *a* is more than overcome by competition of the large number of part-activities involved in solving the varying equations. Robinson and Bills make a rather unwarranted use of the term refractoriness. They state that, "The large biological phenomenon of refractoriness is a direct correlate of the recency and frequency of functioning of the activity in which the refractoriness is manifest" (p. 442). Since we do not as yet know the direct relationship of thought processes to neural phenomena, and since we do not know how stimuli such as writing letters and typing affect the nervous system, nor how extensively the neural mechanism is involved, the preceding statement of Robinson and Bills on which they base conclusions for this study may be somewhat fallacious.

Kræpelin has held that change of work is effective as rest only when the change is to a less difficult task. Are we justified in assuming that the cancellation test is an easier test than is the equation test? The entire problem needs extensive exterimentation of a very analytical nature. At present the discussions of the work curve, the effects of various stimuli on it, the development of the decrement, the initial spurt, the final spurt, and of conditions favorable to best work, are largely theoretical and not based on sufficient experimental evidence to justify their application to practical situations. Problems such as the one undertaken in this study are difficult because of the many factors involved which have not met with experimental analysis. The study itself raises the question of whether the decrement developed in the equation test and not in the cancellation test was really due to the relative difficulty of the two tests or to some other conditions. The type of activity in the cancellation test would seem to be more monotonous and more homogeneous than that in the equation test. Yet we are not able to state just what constitutes monotony and difficulty of a task. Can difficulty be measured in terms (1) of the number of part-activities involved, (2) of the lack of resistence to work decrement, or (3) of its relative degree of unimprovement under practice? According to these criteria the cancellation

test is easier than the equation test; it shows less improvement by practice in the same length of time; it has fewer part-activities; and it is more resistant of decrement due to continuous work. It also seems possible that a monotonous task, such as the cancellation test, might fail to show a work decrement because its activity might become so autonomous as to coincide with certain bodily rhythms. A coördination of rhythmic functions might be a means of re-inforcement so that the activity might be able to be continued over a long period of practice without decrement. The development of a working rhythm is very noticeable in negroes working in teams at digging, woodchopping, or other simple autonomous activities.

In any practical work situation the decrement incurred from continuous work could not be isolated from the effects accompanying change of work. Mathematically, this has been accomplished in this study. Since in any work problem both the effects of continuous practice and the effects of change would be operating at the same time, this study bases its conclusions on a straight comparison of corresponding intervals for the groups that worked continuously and the groups that changed work. Table 5 shows these results for the equation test; Table 8 shows the same results for the cancellation test.

The changes of work as observed in Tables 5 and 6 were having less and less effect in each succeeding interval. However, when the effects of continuous work are not included, the effects of change of work become more noticeable in succeeding intervals as is shown in Table 7. The experiment should be continued over a longer interval of time to see whether the effects of change of work would ultimately wear away, or whether the tendency shown was a mere fluctuation in the work curve. This was not done in this experiment because it was impossible to score the large number of tests while the testing was going on. Hence, this tendency was not noticed until some weeks after the experiment had ceased. The problem of securing so large a number of subjects made a reset of the experiment next to impossible. It may be that this tendency of the continuous work curve to show less and

less decrement was due in the final interval to something like the 'final spurt.' The subjects knew the length of the work period. Such a spurt might not be noticeable if the subject were not aware of the length of time the experiment was to be continued.

Practice gain was taken out in the study by means of computation. No means of computation is ever entirely satisfactory in a condition like this one. The method used was explained in the chapter on results. It was based on the assumption that the practice gain is equally distributed over the four intervals of each day. This is probably not the case. However, had the gain of each interval been compared with the same interval for Monday and practice gain taken out accordingly, the possibility of variations of the different intervals on other days of the week would have been lost. If all scores were reduced to the equal of the Monday scores at each interval, it would have been just as well to consider the tests on Monday only. The plan used would permit relative differences of the various intervals to maintain something of their original relativity. Since the primary interest of the experiment was to study the effects of change of work on the various intervals, it seemed best to adopt a plan for the elimination of practice gain which would cause as little disturbance of the variation from interval to interval as possible. The probability is that the largest practice gain is in Interval 1 of each day. If this should be taken note of, it would probably increase the size of the decrement in the other intervals.

The decrement developed from continuous work (Group I) on the equation test was partially relieved by change of work (Groups II and III) to the cancellation test. Some slight decrement existed in the groups that worked on the equation test but changed work. At least two explanations are possible. On the one hand, a longer interval of work on the cancellation test might allow for a complete recovery of the group and entirely eliminate the decrement. On the other hand, there is a possibility of some overlapping of the two types of function. It is necessary that different kinds of

work receive further investigation and determination before we know what we mean by a 'type' of work. The two activities selected for this study were taken because of their evident difference in 'type' and with the hope of giving a real change of work situation. Dodge (1917) suggested three possibilities of distinguishing types of work on the basis of their relationship to effects of fatigue. The same might apply to the development of the decrement whether or not the decrement is a condition of fatigue. Dodge states that, (1) when fatigue of one mental situation is accompanied by fatigue of another, there must be some dynamic factor common to both. (2) When fatigue of one mental process does not show as fatigue in another, the two must depend on different dynamic conditions, and (3) when fatigue of one process is accompanied by improvement in another, the two processes are probably related in the sense that the fatigued factor in the former was inhibitory to the latter. The first condition seems to have prevailed in the relationship of the equation test to the cancellation test. There is a possibility that the two processes depend on different dynamic conditions, or that there is a slight overlapping of the two processes.

The results of the cancellation test are so contradictory as to call for some explanation. The writer is of the opinion that the groups were not well equated in regard to their ability on the cancellation test. The correlation of the Otis S.A., the criterion used for equating the groups, with the cancellation test was only .25 after the preliminary practices. The results would indicate that Group III was throughout the testing better than Groups II and IV. The reliability of the cancellation test was lower than that of the equation test, being .77 for the cancellation test and .87 for the equation test.

Kincard (1925) summarized the results of numerous studies relative to the effects of practice on the variability within groups. She found that while differences in groups generally become relatively less with practice, the relative standing of the individuals tends to remain the same. The groups working on the equation test and the cancellation

test do not show much variability in their fourth week of practice. Reliability coefficients of the two tests for the fourth week of testing are shown on page 233. The Otis does not have any prognostic value in determining improvement with practice on either the equation test or the cancellation test. The correlations were .02 and .03 respectively.

Summary and Conclusions

This study has attempted to determine whether under certain well controlled conditions there exists a work decrement in two types of mental activity, and if such a decrement exists, if it may be reduced or eliminated by change from one type of mental activity to another. The experiment was made on 80 college women divided into four groups equated on basis of scores made on an Otis Self Administering Test of Intelligence. The tests used were the 'A' cancellation test and the Peterson Uniform Test of Intelligence which is an equation test. One group of subjects worked continuously for 20 minutes at the cancellation test, another group worked continuously at the equation test for the same length of time. The two groups indicated their progress every five minutes. The other two groups alternated work on the two tests, one beginning with the cancellation test and the other with the equation test. Alternations occurred every five minutes. No work decrement developed in the cancellation test, but a decided decrement developed in the equation test.

The results shown by the cancellation test are contradictory among themselves. The second and fourth intervals show a difference of the means of the group that worked continuously and the group that changed work which is favorable to continuous work, while the third interval has a difference in the means of the two groups which is favorable to change of work. The results themselves show that the groups were not equated well as regards the cancellation test. Group III started on Interval 1 well ahead of Group IV, and it is also consistently better than Group II. Groups IV and II seem better equated. Since the reliability of the cancellation test is .77 and its correlation with the Otis S.A. only .25,

it is not surprising that the results are so irregular. The differences between the means of the groups doing continuous work and those changing their work every five minutes are possibly due to a lack of proper equating of the groups, rather than to any difference developing as a result of change of work.

The equation test shows a definite work decrement resulting from conditions of continuous work. Change of work does not entirely eliminate the decrement, but it lessens it to a large extent. The differences in the means of the group working continuously and the means of those changing work are constant in favor of change of work. They are not, however, reliable according to the usual criteria. This is doubtless due to the small number of cases.

When the effects of continuous practice are eliminated from the effects of change of work, the differences are still constant and favorable to change of work.

The greatest difference appears in the second interval of work. This is due to the fact that the greatest decrement for continuous work is in the second interval. The decrement becomes less and less in each succeeding interval, and the differences between the group working continuously and the ones changing work become smaller and less reliable in terms of standard units with each succeeding interval after the second interval.

The intelligence test score was not, in this instance, indicative of ability for practice improvement on the fourth week of practice on either the equation test or the cancellation test.

The probability is considerable that change of work is effective in reducing the work decrement in the more complex types of mental activity. Simpler types of activity do not appear to be as susceptible to decrement under conditions of continuous work.

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SOME QUANTITATIVE CHARACTERISTICS OF PAROTID SECRETIONS*

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From recent investigation of the characteristics of the secretory process, it appears that studies such as have been made of (*a*) the relation of the quantity of secretion to the intensity of the stimulus, or of (*b*) the influence of various types of activity on the rate of secretion, need to be re-evaluated. A fixed stimulus applied at the beginning of an experiment apparently produces distinctly different results if applied again before the gland has opportunity to recuperate. Unless what seems to be a definite limitation of glandular functioning under intense stimulation is considered, the augmented flow which appears in the early stages of excitation of the salivary glands might readily be interpreted as evidence of a cortical or appetite influence, while the subsequent reduced flow might be interpreted as an evidence of fatigue. Nowhere in the literature dealing with glandular behavior have we been able to find studies where this factor has been given consideration. It is the purpose of this report to present data showing the characteristics of parotid secretion as shown in continuous and intermittent stimulation and to indicate some of the implications of these findings.

The most important problem in making such a study is the selection of an adequate stimulus whose excitatory value would remain fairly constant. After some preliminary experimenting it was found that chewing some tasteless object, if carefully timed and localized, produced a more regular rate of secretion than was obtained when acids were used. The

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following brief summary of the literature indicates both the technique and conclusions of investigators who have reported the effects of chewing activity on the rate of secretion.

About one hundred years ago Mitscherlich (1832),[1] a German physician, observing a patient with a parotid fistula of the left Stenson's duct, noted that the secretion increased when the lower jaw was active. Ordenstein (1860) [2] using a small canula to drain the secretion from the duct reported an increased flow during and immediately following a meal. Stoney (1873) [3] concluded from his observations of a healthy young woman with a fistula, that mastication stimulates the flow of saliva to a considerable extent. Tuczek (1876),[4] weighing the food before ingestion and then again before swallowing to determine the total quantity of saliva secreted under various situations, reported that chewing, speaking, and opening the mouth excite secretion. Küss (1899) [5] observing another patient with a fistula reported that chewing movements with mouth empty increased the rate of secretion from .8 to 20, cc per hour. Zebrowski ('04) [6] made an extensive study with two subjects having parotid fistulas and observed that chewing food on one side caused a greater secretion on that side than on the opposite side; that the glands secreted profusely during the first ten minutes of eating and then the flow subsided; and that each swallow caused an increase in the saliva. Brunacci ('10) [7] in a series of investigations with a canula, obtained from 7 to 15 drops of saliva per minute when the subject chewed tasteless rubber.

[1] C. G. Mitscherlich, Ueber den Speichel des Menschen. Poggendorffs Analen. d. Physiol. und Chemie, 1833, 27, 320–345.

[2] L. Ordenstein, Ueber den Parotidenspeichel des Menschen. Beitr. Z. Amat. u.i Physiol., 1860, 2, 103–122.

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[5] G. Küss, Notes sur la saliva parotidienne de l'homme. *Jour. de l'Anatomie*, 1899, 35, 246–253.

[6] E. V. Zebrowski, Zur Frage der sekretorischen Funktion der Parotis beim Menschen. Arch. f. d. ges. Physiol., 1905, 110, 105–173.

[7] B. Brunacci, Sulle funzione secretoria della parotid Nelluoma. Arch. di Fisiol., 1910, 8, 421–457.

Lashley ('16) [8] using his suction disc for collecting the saliva, made a study of the factors affecting parotid secretion and observed among other things that chewing a foreign object evoked increased secretion. The augmented secretion during the first minutes of excitation was ascribed by this author to stimulation factors. Krasnogorski ('26) [9] employing a disc cup, as well as having access to a patient with a fistula, observed the influence of mechanical and chemical stimulation of different areas of the oral cavity. His only report on chewing was to note its direct effect already suggested by Zebrowski and Lashley. He also pointed again to the limitations of the use of the fistula as a reliable means of obtaining secretion. In some earlier studies [10, 11] Windsor confirmed the observations noted above that chewing a foreign object excites the parotid gland to activity and that this action has all of the characteristics of a direct reflex.

Technique

To collect the secretion a suction disc with an enlarged vacuum chamber was placed securely over the mouth of Stenson's duct. From the disc a small rubber tube drained the secretion through the corner of the mouth to a graduate tube in one experiment and to an electric drop recorder in the other. Since the chewing process itself has been found to evoke little secretion it was necessary to provide some object to be chewed in order that maximal glandular activity would obtain. After experimenting with a variety of objects, tasteless gum was selected as the most satisfactory material for this purpose. It has the advantage of fairly uniform consistency and volume and approaches the natural food situation more nearly than other objects suggested. A definite bolus of

[8] K. S. Lashley, Reflex secretion of the human parotid gland. *Journal of Experimental Psychology*, 1916, 1, 461–493.

[9] N. I. Krasnogorski, Ueber die Wirkung Mechanischer und Chemischer Reizungen verschiedener Teile der Mundhöle auf die Tatigkeit der Speicheldrusen bei Kindern. Jahrbuch f. Kinderheilkunde, 1926, 114, 256–269.

[10] A. L. Winsor, Conditions affecting human parotid secretion. Journal of Experimental Psychology, 1928, 11, 355–363.

[11] A. L. Winsor, and T. L. Bayne, Unconditioned salivary responses in man. American Journal of Psychology, 1929, 41, 271–276.

this gum was chewed between the molars on the side from which the secretion was being drained. The chief problem with such a stimulus is to keep it properly localized. Owing to differences in sensitivity of different areas of the mouth changes in location effect pronounced changes in the volume of secretion. To partially control this factor the subjects practiced chewing the gum in the proper position at a definite rate until the gland maintained an even flow. Although changes in pressure in the mastication process appear to have little if any effect upon the rate of secretion, each subject was instructed to maintain a normal chewing pressure and to keep his lips closed. He was to swallow at a given signal once every minute. The rate of chewing was timed by a metronome and the pauses by a stop watch. Readings were made in terms of ccs at the end of each minute for the first experiment reported and in terms of drops per minute for the second.

Results

The first problem was to determine the characteristics of the flow when the chewing was maintained at a uniform rate over a protracted period. Figure 1 is a typical graphic record of the amount of secretion flowing from the parotid duct each successive minute when the rate of chewing was one chew per second. It will be noted that there is but slight variation in the various columns after the first two minutes. Although the subject an adult chewed for fifty minutes in this case the diminution toward the end of the period was not marked. Attention, however, is immediately attracted to the unusual increases in the flow for the first two minutes. Note that although the stimulus for any two minutes is comparable, there is a difference between the rate of secretion for the first two minutes and any other two later in the series. This characteristic of glandular functioning is more apparent when rest periods are introduced.

In Fig. 2 the dark columns represent the volume of secretion obtained per minute when the subject was chewing at the same rate as in Fig. 1, and the stippled columns represent the secretion obtained during the intervening rest periods when

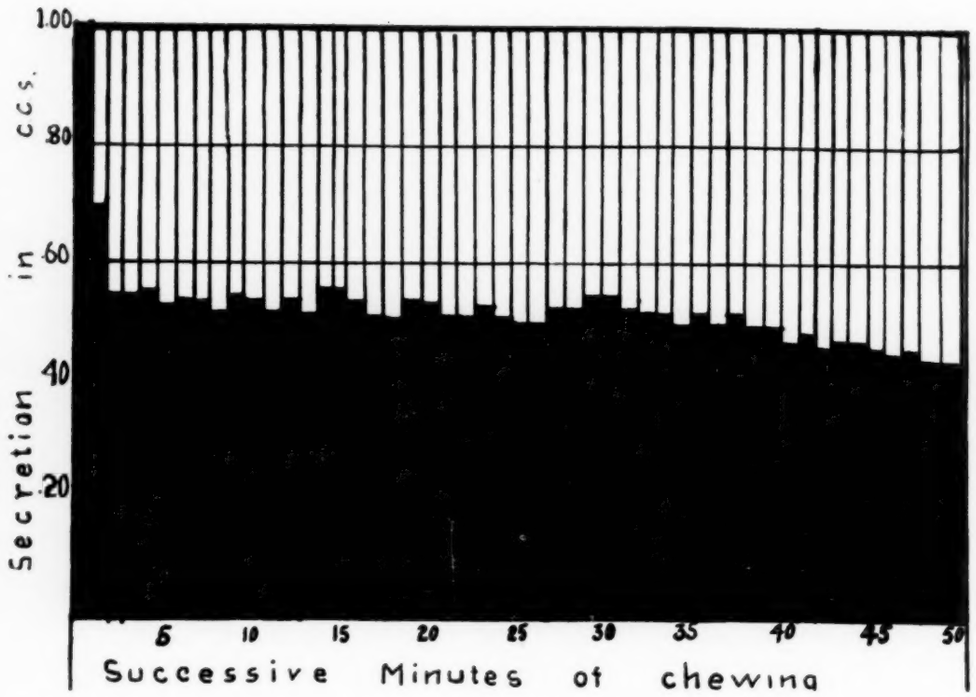

FIG. 1. The effect on parotid secretion of chewing tasteless gum at the rate of one chew per second.

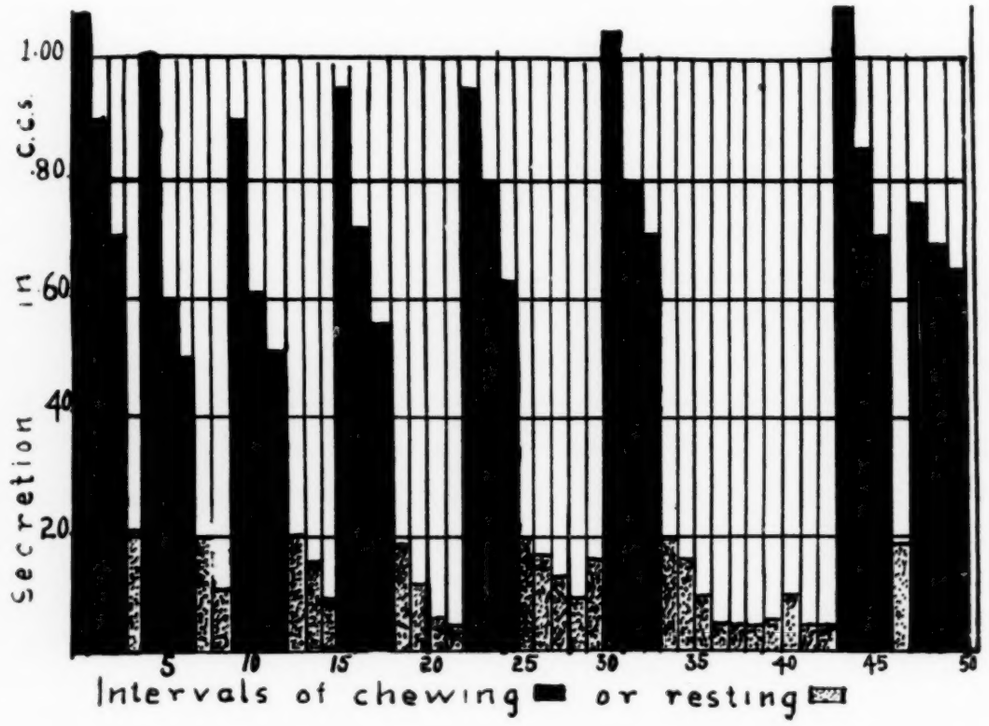

FIG. 2. The effect of pauses of varying duration on the quantity of saliva secreted.

the gum was removed. The procedure adopted in this test was to have the subject chew for three minutes and then rest. The duration of the rest periods was gradually increased up to ten minutes. This was followed by an interval of thirty seconds shown as the last interval in the figures.

It will be noted that the first minute of stimulation following each interval is characterized by an augmented flow of saliva, similar to that noted in Fig. 1. The second and third minutes of the chewing period show a tendency for the volume to decrease rapidly but the extent of the diminution appears to be closely related to the duration of the pause. As the successive pauses are lengthened the amount of secretion for the second and third minutes gradually increases. After the thirty-second interval the volume for the first minute was definitely reduced. Following the five-minute interval the gland appears to be able to secrete approximately as much as in the first exercise.

It is apparent from these data that the distribution of stimulation is significant in glandular activity. The rate of chewing would, therefore, be expected to be significant in determining the rate of secretion. A slower rate of chewing would be expected to be biologically more economical and effective than a more rapid rate. To check this assumption and at the same time indicate a practical application of the characteristics of secretion under discussion an experiment was arranged to show the effect of different rates of chewing on parotid secretion.

Except for the method of recording and the variation in the rate of chewing, the procedure was the same as that described in the previous experiments. Instead of the secretion falling into a graduate as above each drop fell on the delicately adjusted lever of a Macmillan drop recorder. An electric signal key in circuit with the drop recorder, registered each drop on a slowly revolving kymograph. An inflated rubber bulb fastened securely under the subject's chin was connected by a rubber tube to a Marey tambour. The lever of the tambour was in circuit with a second signal key which registered the chewing movements. A graphic chronometer

served as a time-marking device. All evidence of food was removed from the experimental room and to avoid the effects of dehydration the subject was requested to drink a pint of water a few minutes before the tests were made.[12] A five-minute rest period preceded the making of the records.

Figure 3 represents a kymographic record of the number of drops of saliva produced when an adult subject was chewing at different rates. Five different rates were used and the records represent but one minute at each rate. The breaks in the upper line of each record indicate drops of saliva and the breaks in the second line indicate chewing movements. In the interest of space all time records except that on the lower record were cut away. Record A shows the number and distribution of drops when the interval between chewing movements was 20 seconds. Record B shows intervals of 10 seconds; C intervals of 5 seconds; D intervals of 1 second, and E intervals of ½ second. Short pauses preceding D and E account for the increased secretion at the beginning of these charts.

These records show that the volume of secretion per minute is influenced by the rate of chewing. The slower the rate of chewing the more effective is each single mastication movement in exciting secretion. If the first twenty seconds of each record is eliminated so that the influence of the preceding rest period is reduced, the average number of drops per chew for the other forty seconds is as follows:

A = 7.	drops	per	chew
B = 4.	"	"	"
C = 2.62	"	"	"
D = .42	"	"	"
E = .23	"	"	"

Chewing forty times as frequently in E as in A increases the number of drops per minute only 75 percent. As far as bodily economy is concerned the slower rate of chewing has obvious advantages. The optimum rate of sustained chewing

[12] A. L. Winsor, The effect of dehydration on parotid secretion. *American Journal of Psychology* (in print).

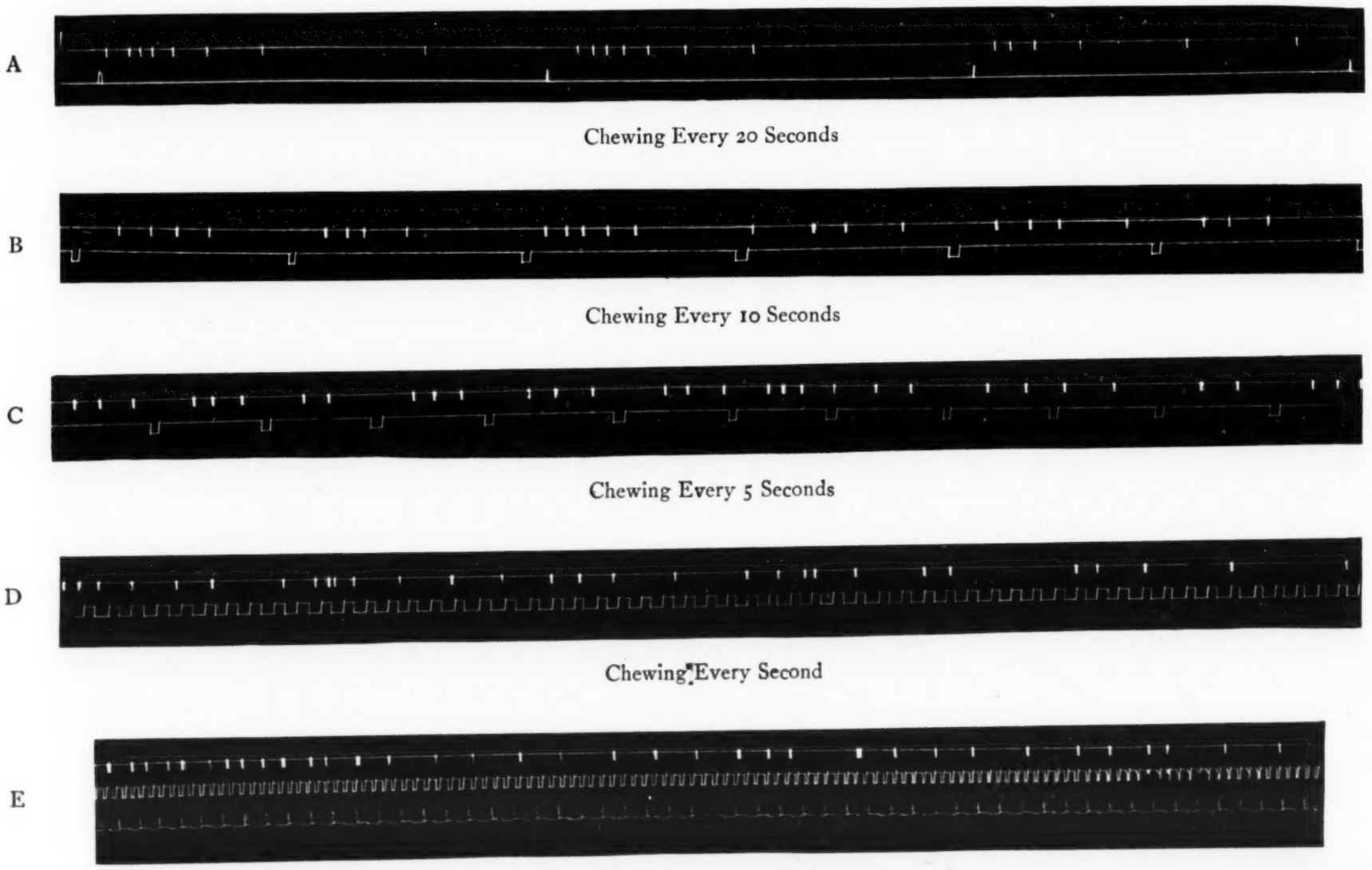

FIG. 3. The effect of different rates of chewing on the rate of parotid secretion.

which provides the most secretion per minute over an extended period was found to be one chew each five seconds for this subject.

Discussion

To explain the phenomena noted here a general idea of the physiology of secretion is necessary. The secretory activity of the salivary glands is always associated with a considerable dilatation of the vessels of the gland, and a consequent large increase in the blood flow through the gland, which may amount to between three and eight times the quantity during rest. From the blood vessels the fluid passes first into the lymphatic spaces where it is taken up by the secretory cells. The first effect of the stimulation appears to be on these cells. In spite of the increase in the size of the vessels, an actual diminution in size of the gland takes place. This would indicate that under this first stimulation the cells of the alveoli discharge the fluid they contain.

Such a process would have distinct limitations under rapid stimulation and it would seem highly probable that this mechanical limitation accounts to some extent at least for the phenomena observed in the figures. The initial augmented flow shown in Figs. 1 and 2 at the beginning of the chewing exercises and following rest periods, would seem to be due to a full supply of liquid in the gland when the stimulation occurred. Subsequent reduction under frequent stimulation would be explained in terms of a temporary reduction of the liquid supply available in the gland. The level of secretion maintained after the first two minutes of stimulation shown in Fig. 1 would represent an optimum secretion for the rate of stimulation used after this reserve supply of the glands has been utilized. Introduction of an interval without stimulation would permit a restoration of the reserve supply and the longer the rest period the more complete the replenishing process. According to Fig. 2, a five-minute interval seems to be about long enough for complete recovery of maximal strength of the glandular activity. The extent to which a gland has been active or inactive preceding a given stimulation partially determines the volume of secretion obtained from

the excitation for at least two minutes at the beginning of the secretory process.

It seems apparent from these data that failure to consider the distribution of stimulation or the activity of the gland immediately preceding the tests would invalidate any experiment in which the quantity of the response was a factor. Whether or not this mechanical factor is responsible for some of the augmented secretion obtained at the beginning of a conditioned response is a problem for future research. The implications of this study to the science of nutrition are obvious.

Summary

Parotid secretion during prolonged chewing activity is characterized by a pronounced initial flow followed by a reduced but relatively constant rate for a period of at least one hour. When pauses of varying duration occur intermittently with the chewing activity the rate of secretion is materially affected. Within certain limits the longer the rest periods preceding excitation the more rapid the subsequent rate of secretion. A slow rate of chewing is, therefore, biologically more economical than a rapid rate. Chewing forty times as rapidly at one time than another does not even double the amount of secretion obtained for the same period of time. For the subjects investigated the optimum rate for a ten-minute period was one chew every 5 seconds.

This factor of the effect of distribution of stimulation on the rate of secretion appears not to have been considered in studies involving the parotid glands.

(Manuscript received June 12, 1930)

AN EXPERIMENTAL STUDY OF RETENTION IN THE WHITE RAT

BY STEPHEN MAXWELL COREY[1]

DePauw University

Introduction.—The ability of an animal to retain the behavior patterns which it has learned is certainly as interesting and suggestive a subject for study as is the speed with which the patterns were originally learned. Schenk and Warden,[2] however, have pointed out that the experimental investigation of retention stands far behind experimental studies of learning. In recognition of this fact, the following is a study of the similarities and differences between the learning and relearning scores of white rats on an elevated maze.

Judging from the experimental literature, Brockbank [3] and Tsai [4] appear to be the only ones who have seen in retention a problem of major importance. Small,[5] Bassett,[6] Hubbert,[7] Hunter,[8] and Ulrich [9] have reported incidental observations made in the course of researches on other topics.

Most of these studies on retention in the rat suffer from criticisms to which so many comparative investigations are

[1] From the Laboratories for Research in Athletics, University of Illinois.

[2] Schenk, M. R., and Warden, C. J. A comprehensive survey of the experimental literature on animal retention. *J. Genet. Psychol.*, 1930, 36, 1, 20.

[3] Brockbank, T. W. Redintegration in the white rat. *Behav. Monog.*, 1919, 4, No. 1.

[4] Tsai, C. Comparative study of retention curves for motor habits. *Comp. Psychol. Monog.*, 1924, 2, 1–29.

[5] Small, W. S. An experimental study of the mental processes of the white rat. *Amer. J. Psychol.*, 1900, 11, 131–165.

[6] Bassett, G. C. Habit formation in a strain of white rats of less than normal brain weight. *Behav. Monog.*, 1914, 2, No. 4.

[7] Hubbert, H. B. Habit formation in the white rat. *Behav. Monog.*, 1915, 2, No. 11.

[8] Hunter, W. S. Auditory sensitivity in the white rat. *J. Anim. Behav.*, 1919, 5, 312–319.

[9] Ulrich, J. L. Distribution of effort in learning in the white rat. *Behav. Monog.*, 1915, 2, No. 10.

susceptible. The first of these has to do with the number of subjects used. With the possible exception of the work done by Hubbert and Ulrich, none of the studies on retention is based upon enough subjects to warrant very far reaching conclusions. Small used three subjects, Hunter four, Brockbank four to ten per group, and Tsai ten to twelve. Furthermore, a number of the studies are reported in such general terms that it is practically impossible to ascertain just what the experimenter did. In some of them, it seems, too much was attempted. The investigator concerned himself with three or four problems, and hence his conclusions to any single problem were apt to be complicated by irrelevant factors. In view of these criticisms an attempt has been made in the following report (i) to use a significantly large number of subjects, (ii) to outline carefully each step in the experimental technique, and (iii) to limit the treatment to the one major problem of retention.

Subjects and Apparatus.—186 Albino rats from Wistar stock were used in the study. They were housed in a cement floored room, free from wild rodents and insects. The laboratory was disinfected regularly, and the temperature thermostatically controlled. Food consisted of grain, cheese, milk, lettuce, dog-biscuit, and a weekly dose of cod liver oil. The subjects were in the best of health throughout the study, and they were handled by the experimenter only.

The learning ability of the rats was tested by having them solve an eight cul-de-sac, elevated, skeleton maze, very much like the one reported upon by Miles.[10] The particular maze used in this study has proved to be highly reliable. Its particular advantages have been reported upon elsewhere.[11]

Procedure.—Experimentation was started when the subjects were sixty days old. One week was taken up in an attempt to orient the animals to the maze-food situation. During this period the rats were fed on the maze in accordance with the following schedule:

[10] Miles, W. R. The narrow path elevated maze for studying rats. *Proc. Soc. Exper. Biol. and Med.*, 1927, 24, 454–456.

[11] Corey, S. M. The reliability of the elevated skeleton maze. *Amer. J. Psychol.*, 1930, 42, pp. 439–442.

Day	Number of Subjects on the Maze at One Time	Culs-de-sac on Which Food Was Placed
1st	An entire litter	All
2nd	An entire litter	All
3rd	An entire litter	All
4th	One individual	1, 3, and 6
5th	One individual	2, 4, and 7
6th	One individual	5 and 8

This relatively long period of orientation served to give reasonable assurance that the subjects would start seeking food as soon as actual experimentation commenced. By the end of this first week each rat had obtained food at least once from every cul-de-sac. By the fifth day of orientation the behavior of the animals indicated clearly that the maze 'meant' food, although the random nature of the exploratory movements showed an absence of any particular behavior patterns that might have been acquired during orientation.

During experimentation the subjects were given five runs on the maze per day, Sundays included. Although Brockbank,[12] Yerkes,[13] Hunter,[14] Ulrich,[15] Cook, [16] and Warden [17] have demonstrated that a greater distribution of practice results in more rapid learning, this factor was not considered particularly pertinent to the problem under investigation. Furthermore, a greater distribution might have introduced age factors which would have complicated our findings. The norm of mastery for the maze was five consecutive, errorless trials. Speed of learning was judged in light of the following three criteria:

(i) Total number of errors made before the maze was learned,
(ii) Number of trials required before five consecutive, errorless runs had been completed, and
(iii) Total number of seconds spent on the maze.

[12] Brockbank, T. W., *op. cit.*

[13] Yerkes, R. M. The Dancing Mouse. 1907.

[14] Hunter, W. S. The delayed reaction in animals and children. *Behav. Monog.*, 1913, No. 6.

[15] Ulrich, J. L., *op. cit.*

[16] Cook, S. H. The effect of various temporal arrangements of practice on the mastery of an animal maze of moderate complexity. *Arch. Psychol.*, 1929, No. 98.

[17] Warden, C. J. Distribution of practice in animal learning. *Comp. Psychol. Monog.*, 1923, No. 3.

The errors recorded for learning were of one type only, namely, those which involved a complete entrance into a cul-de-sac while the subject was running in a forward direction. Entrance into a cul-de-sac made while the rat was retracing its path did not count as an error, although a separate record was made of every such digression. As soon as the correct orientation was again assumed, any cul-de-sac entered counted as an error.

Fourteen days after the maze was learned it was relearned. The norm of mastery as well as the criteria for speed of learning were the same for both learning and relearning. A conscientious attempt was made to equate motivation for the learning and relearning performances. The reward for each was a small piece of milk-soaked bread the size of which did not vary from trial to trial. Food was removed from the nest cages four hours prior to maze running for both learning and relearning. The assumption seems justified that the subjects were equally motivated while the maze was being learned and relearned, save for unavoidable changes in age, œstrous cycle, and the like.

Bases for Comparing the Learning and Relearning Scores.—A comparison was made of the learning and relearning scores with respect to the following particulars:

(i) Speed and variability of performance as judged by error, trial, and time scores,
(ii) Location of errors,
(iii) Ratio between the total number of errors made and the number made more than once by the same subject on one day (repeated errors),
(iv) Ratio between the total number of errors made and the number of retracements, and
(v) Relationship between the learning and relearning scores as measured by the Pearson product-moment coefficient of correlation.

Comparisons of Error, Trial, and Time Scores.—Table I gives the mean learning and relearning scores for the 186 subjects. As might be expected, the animals relearned the

TABLE I

LEARNING AND RELEARNING CRITERIA SCORES

Criterion	Learning		Relearning	
	Mean	S.D.	Mean	S.D.
Error	40.15	22.15	12.13	10.46
Trial	32.75	16.59	16.34	10.30
Time	2064.09	1888.70	811.96	794.26

maze with much greater rapidity than they were able to learn it. The shape of the learning and relearning curves for all criteria was practically the same. In so far as a judgment can be made, and keeping in mind that different norms of mastery as well as different mazes were used, the degree of retention of Bassett's subjects was greater than that of the subjects in the present study, even though his retention interval was sixty days. Tsai's subjects too, retrained after a seven day interval, relearned the maze with much greater relative rapidity than did ours. The relative variability of the relearning scores was somewhat greater when compared to that of the learning scores. Table II gives the Pearson coefficient of variation for both learning and relearning error, time, and trial scores. In each instance the learning scores were less variable. This tendency was most noticeable with respect to the error scores.

TABLE II

COEFFICIENTS OF VARIATION FOR LEARNING AND RELEARNING SCORES

Criteria	Learning	Relearning
Error	55	86
Trials	51	63
Time	91	98

Location of Errors in Learning and Relearning.—In view of the type of maze used, it is evident that a subject might err by entering any one or more of the eight culs-de-sac. As those nearest the start were the last to be eliminated, it was thought that learning and relearning error scores might differ with respect to their location. Table III gives the percentage of the total number of errors made which occurred on any one

cul-de-sac. As Brockbank [18] has shown, there seems to be no difference with respect to this aspect of learning and relearning performances. The rank coefficient of correlation between the two variables is .96.

TABLE III

PERCENTAGE OF TOTAL NUMBER OF ERRORS THAT OCCURRED ON ANY ONE CUL-DE-SAC

Cul-de-sac	Learning	Relearning
1	16.5	16.3
2	16.9	19.8
3	22.2	26.1
4	11.3	11.4
5	12.5	11.3
6	5.6	5.0
7	10.3	8.7
8	4.7	1.4

Retracements. As the writer considers retracing to be a natural phase of maze learning, no backstops were placed on the pathways to eliminate this type of behavior. Furthermore, as retracing seems to be a rather unique tendency, the learning and relearning performances were compared with respect to the number of such movements made. Table IV gives the ratio between the total number of errors made and the number of retracements for both learning and relearning performances. The index is very nearly the same in each case, although the subjects showed a slightly stronger tendency to retrace during the relearning.

TABLE IV

RATIO BETWEEN TOTAL NUMBER OF ERRORS MADE AND TOTAL NUMBER OF RETRACEMENTS

Learning	.052
Relearning	.068

Repeated Errors.—As practice was distributed in groups of five trials per day, it was possible for a subject to err on the same cul-de-sac a number of times each day. As this repetition of errors seemed to indicate a lack of learning ability, it was also used as a basis for a comparison of learning and relearning scores. Table V gives the ratio between the total

[18] Brockbank, *op. cit.*

TABLE V

RATIO BETWEEN TOTAL NUMBER OF ERRORS AND TOTAL NUMBER OF REPETITIVE ERRORS

Learning	.278
Relearning	.172

number of errors made and the total number of repetitive errors for both learning and relearning. The subjects repeated errors with relatively greater frequency during learning than during relearning.

Correlation Between Learning and Relearning Scores.—Granted that animals relearn a maze more rapidly than they are capable of learning it, another question arises, viz. are the rapid learners also rapid relearners, and vice versa? An attempt was made to find an answer to this problem by computing the Pearson product-moment coefficient of correlation between learning and relearning scores for all subjects. Table VI gives these coefficients for error, trial, and

TABLE VI

CORRELATION BETWEEN LEARNING AND RELEARNING SCORES

Criteria Correlated	Coefficient of Correlation
Errors	.16±.05
Trials	.19±.05
Time	.49±.04

time scores. The values are quite low and statistically insignificant for the first two criteria of speed of learning. The relationship between time scores is somewhat higher, but time is the poorest of the three as a measure of speed of learning.[19] If a subject was slow and ran lethargically during learning, it would probably do the same during relearning, regardless of the errors made at the different times. This would result in a higher correlation between learning and relearning total time scores and yet would have no necessary bearing upon learning ability.

Conclusions.—In light of the subjects and techniques used in the study the following conclusions concerning retention seem justified.

[19] See, Warden, C. J., and Hamilton, E. F. The effect of variation of length of maze pattern upon rate of fixation in the white rat. *J. Genet. Psychol.*, 1929, **30**, 235. Corey, S. M., *op. cit.*

1. Relearning was accomplished with significantly greater rapidity than was learning, although the shape of the learning curves was much the same for each.
2. The variability of relearning scores was relatively greater than that of learning scores.
3. The relative difficulty of various parts of the maze was the same for learning and relearning.
4. The number of repetitive errors as compared to the total number of errors made was greater for learning than for relearning.
5. The number of retracements as compared to the number of errors was approximately the same for learning and relearning.
6. The relationship between speed of learning and speed of relearning as measured by a coefficient of correlation was insignificant.

(Manuscript received May 28, 1930)

THE INFLUENCE OF REPETITION AND DISUSE UPON RATE OF HYPNOTIZATION [1]

BY ROBERT G. KRUEGER

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In Hull's outlines of experimental procedures in the investigation of hypnotic suggestion, he remarks:[2] "It is a well known fact . . . that successive repetitions of the trance usually facilitate the induction of the trance on a later occasion. The extent to which this is true and the shape of the practice or learning curve of this particular form of habit may readily be determined." This sets the first problem of the present investigation. A second problem which it was found possible to investigate simultaneously was "whether the increased susceptibility to the trance acquired through repetition is, like ordinary habits, lost through disuse." [3]

Apparatus

Eyelid closure was used as the criterion of the onset of the trance. An apparatus was designed to produce a graphic record of the movement of the eyelid in order to determine accurately the instant at which the lids close. Because of the speed with which the eyelid moves and the small amount of energy available, it was necessary to have a very light recording device which would operate with a minimum amount of friction, and be of such a nature as not to interfere with the subject's falling into the trance. With this in mind the apparatus shown in Fig. 1 was developed.

A fine capillary glass tube (*C*) 16 cm long and ½ mm in diameter, one end of which was fastened to the timer with a few drops of cement, served as a recording stylus for the lid

[1] The writer wishes to express his indebtedness to Professor Clark L. Hull for his helpful suggestions and criticisms during the course of the investigation.

[2] C. L. Hull, Quantitative methods of investigating hypnotic suggestion. *J. Abn. and Soc. Psych.*, July 1930.

[3] Hull, *Ibid.*, Problem No. 24.

movements. A small piece of tin foil (*T*) about 5 mm square was attached to the subject's eyelid with a drop of cement. A fine silk thread fastened to the tin foil rose vertically to the glass ring (*H*) passed through the ring (*H'*), the guide ring (*F*) and thence to the glass marker. A downward movement of the lid consequently produced an upward movement of the stylus. Glass was used for the rings in order to minimize

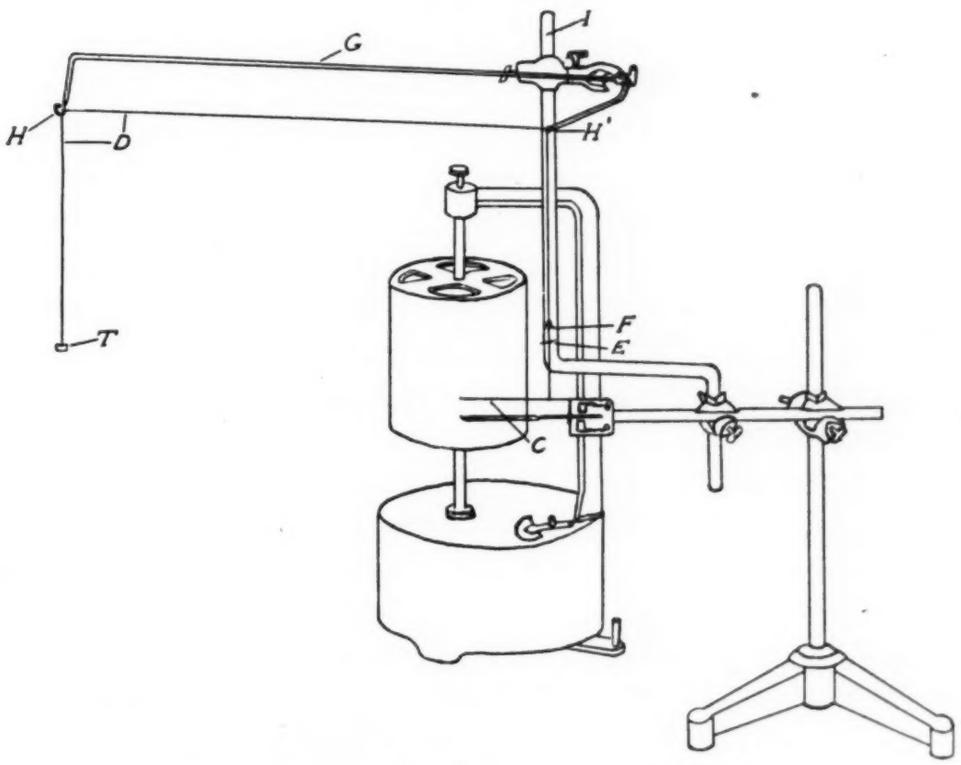

FIG. 1. Apparatus used in recording lid movements.

friction. A universal clamp interposed between rods (*G*) and (*I*) permitted (*G*) to be placed in any desired position. In order to prevent breaking the glass marker by too great an excursion, a pin (*E*) was fastened to the thread below the guide (*F*). In case of a gross downward head movement, the movement of the thread is interrupted by the pin striking the guide, causing the tin foil to come off the subject's lid.

The time and signal markers were combined into a single unit by mounting an electro-magnet on each side of the armature. One magnet was wired to the time clock, the other to the signal key. In the normal position the armature support-

ing the stylus would be midway between the two magnets, marking a straight line on the moving smoked paper. Time intervals are accordingly indicated by deviations below this normal line, and signal marks by those above. The marker and the glass stylus were mounted on a universal standard. This made possible the fine adjustment of the lid-tracing stylus to the smoked paper, which was necessary in order to prevent undue friction. A representative smoked paper record is shown in Fig. 2.

Procedure

Thirteen university students in all were used as subjects, seven in Part I, and six in Part II. They were obtained from the University Employment Bureau and paid by the hour for their services. Two of the subjects of Group I left the University between semesters, so that the second section of Part I was completed with five subjects only.

The subjects, one at a time, were brought into the room and seated in a Morris chair. They were instructed to assume as comfortable a position as possible without sliding down into the chair too far. The small piece of tin foil was attached with Du Pont Household Cement to the center of the upper right eyelid just above the lash, and the apparatus so adjusted that the thread rose in a vertical line from the eyelid. The head was tilted back so that the thread did not touch the eyebrow.

The optical fixation method with the necessary suggestions was used for inducing the trance. The fixation object was a glass rod 25 cm long and 8 mm in diameter with a glass sphere 15 mm in diameter blown at one end. The sphere was filled with mercury which made it an excellent mirror and caused the light to be reflected from a single point upon which the subject fixated. The reflecting ball was held about ten inches from the subject's eyes, and somewhat above the normal line of vision. The suggestions given were maintained as constant as possible throughout the entire experiment. They were as follows:

> Relax completely and be perfectly at rest. Relax all the muscles in your legs, in your arms, in your face, and all over your body. Soon your legs and arms will begin to feel heavy, and your eyelids will become heavy. You will become tired and it will

be increasingly difficult to keep your eyes open. You will want to close your eyes and go sound asleep. It will be so easy to close your eyes, and you will go sound, sound asleep. Your eyelids are becoming heavy, they are getting heavier and heavier, and your eyes are closing farther and farther. You are going sound, sound asleep, sound, sound asleep, etc, etc.

These suggestions were continued until the subject's eyes closed, showing that he was passing into the trance. The duration of the suggestions was measured from the time that the suggestion was begun until the subject completely closed his eyes. This period was used as the basis for plotting the curves.

To be as certain as possible that the trance was really induced, eyelid catalepsy was attempted for the first three trance periods. The subjects, when in the trance, were told that their lids were tightly closed and that they could not open them. The test was regarded as successful if the subjects did not succeed in opening their eyes when displaying the ordinary signs of vigorous effort to do so. This was a minimum requirement that all the subjects had to pass. Most of the subjects went into a moderately deep trance.

The subjects were allowed to remain in the trance for a period of two minutes.

The trance was terminated by the following formula:

I will now slowly count to three and then snap my fingers. When I do you will wake up and you will feel fine. You will be wide awake and not the least bit tired. and you will feel greatly refreshed as a result of the experiment.

This was always repeated twice before the fingers were snapped, so that the subjects could not misunderstand. After a subject was awakened he was dismissed for the day.

As intimated above the experiment was performed in two parts. In Part I the interval between trances was at least twenty-four hours. The time varied somewhat between subjects, but was maintained as constant as possible for each individual subject. The period of no-practice was approximately ninety days. In Part II the interval between trances in all cases was five minutes and the no-practice period was twenty-four hours.

In Part I the length of time required by the subjects to go into the trance varied on the first trial from 100 to 590 seconds.

In Part II, however, it was necessary to select subjects who went into the trance in a shorter time in order to prevent fatigue, since the subjects were put into, and were brought out of, the trance ten times during each experimental period. Accordingly the average trance-induction time in Part II is only about half that for Part I.

Results

There are two rather distinct ways in which the lid responds when the subjects go into the trance. The most common manner is the gradual closing of the lids in which case there is almost a complete absence of winking. A slightly less common manner is a quick closure of the lids preceded by an abnormal amount of winking and quivering of the lids. Such a record is reproduced as Fig. 2.

Subject H
2/24/30
Record No. IA

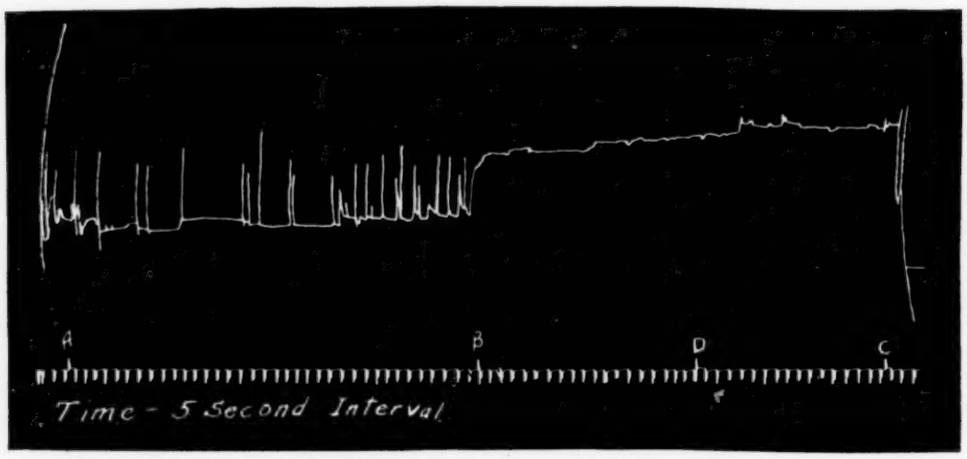

Fig. 2. Typical record of lid movements on going into the trance. (*A*) Beginning of trance suggestions. (*B*) Complete closure of the lids. (*D*) S told that his lids are tightly closed and to try to open them. (*C*) Suggestion given to wake followed at once by the opening of the lids.

Table I shows the length of time suggestion was required in order to induce the trance when repeated at intervals of not less than twenty-four hours. It may be seen that the initial period for inducing the trance varied, but the proportional rate of decrease of time was fairly similar for the several

TABLE I

THE NUMBER OF SECONDS REQUIRED ON SUCCESSIVE DAYS TO PRODUCE LID CLOSURE BY HYPNOTIC SUGGESTION

Trial Number	Time in Seconds Required for Lid Closure						
	Subject B	Subject D	Subject F	Subject H	Subject S	Subject W	Mean Time
1.....	100	590	438	570	305	590	432
2.....	90	443	292	497	270	440	338
3.....	50	435	277	525	222	275	297
4.....	50	372	200	317	165	170	212
5.....	25	309	77	282	165	165	171
6.....	23	330	117	298	90	133	165
7.....	23	229	149	274	109	112	149
8.....	18	197	155	220	94	82	127
9.....	13	200	87	205	87	70	110
10.....	10*	185	130	156	73	85	106

* Extrapolated value.

subjects. Individuals whose first trance time was long, finished the series with a correspondingly long time. Figure 3 is a composite curve presenting graphically the averages

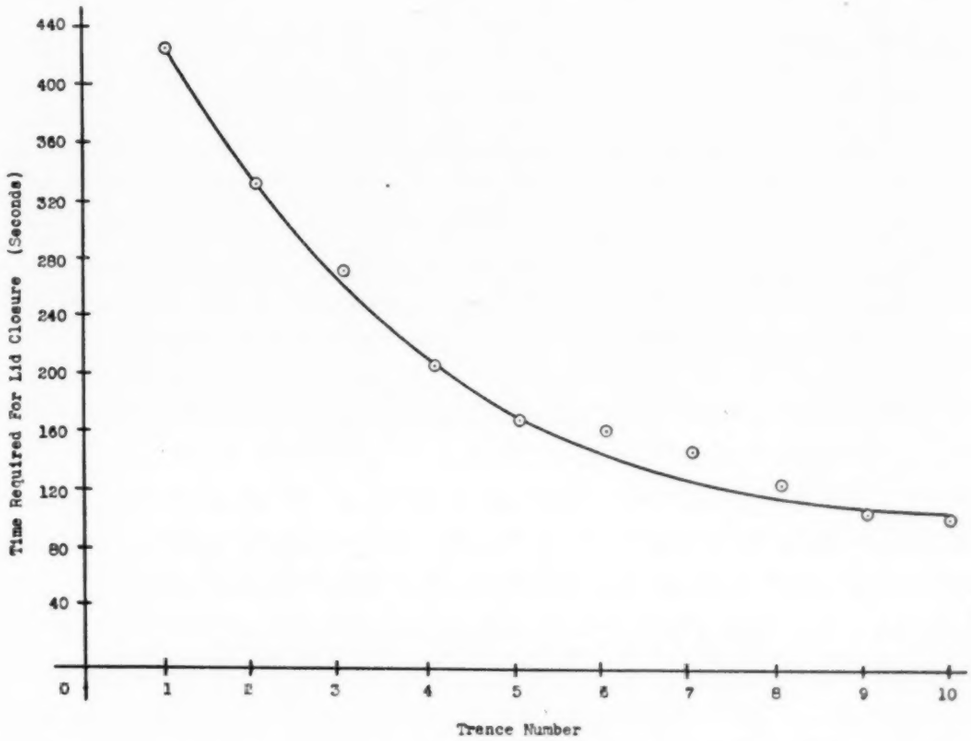

FIG. 3. Composite curve from six subjects showing practice effects resulting from repeated hypnosis. At least 24 hours intervened between successive trances.

in the last column of Table I. This shows that the rate of decrease in time is rapid at first, gradually becomes less and less, and finally approaches a horizontal line.

Table II shows the trance times when induction took place at intervals of five minutes, with a twenty-four hour interval between the tenth and eleventh trances. Figure 4 is a com-

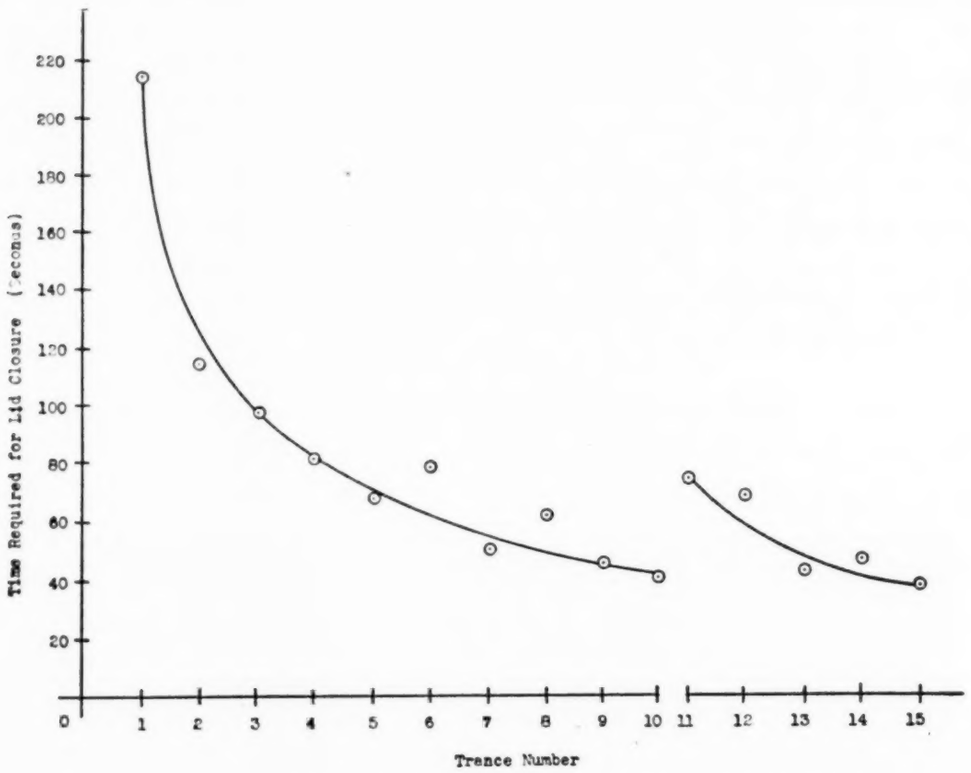

FIG. 4. Composite curve from seven subjects showing practice effects resulting from repeated hypnosis. The intervals between trances 1–10 and between 11 to 15 were five minutes in each case. That between 10 and 11 was 24 hours.

posite curve representing graphically the means contained in the last column of Table II. The general form of the first section of the curve is almost identical with that of Fig. 3. In this case it is especially to be noted that there is an appreciable increase in the trance time following the no-practice period, and that the rate of decrease of the second section is greater than that of the first, as indicated by its steeper fall. At the end of the first practice period the mean time required to induce the trance was 42 seconds. After the twenty-four hour no-practice period the mean time increased to 74 seconds.

In the second practice series five trance periods were necessary to reach the 42-second level, whereas originally seven trance periods were necessary to cause the same amount of decrease in time.

TABLE II

THE NUMBER OF SECONDS REQUIRED TO PRODUCE LID CLOSURE BY HYPNOTIC SUGGESTION

Five-minute Intervals Between Trances

Trial Number	Time in Seconds Required for Lid Closure							
	Subject A	Subject H	Subject C	Subject N	Subject S	Subject Tr	Subject Ta	Mean Time
1.....	45	102	500*	560*	74	40	180	214
2.....	17	55	230	335	67	30	70	115
3.....	6	68	238	235	48	20	55	96
4.....	6	47	206	215	40	10	45	81
5.....	5	40	168	162	26	7	65	68
6.....	3	32	242	175	31	6	66	79
7.....	3	33	170	100	23	5	30	52
8.....	3	43	215	85	16	5	58	61
9.....	2	32	155	65	19	3	47	46
10.....	3	37	140	55	15	3	40	42
Twenty-four Hour Interval								
11.....	8	38	165	188	19	17	82	74
12.....	6	20	140	215	26	6	69	69
13.....	3	24	110	102	20	4	35	43
14.....	3	22	130	115	13	3	35	46
15.....	3	20	112	70	10	4	39	37

* S was told to voluntarily close eyes by E, after which eyelid catalepsy was obtained.

Table III is in part a re-tabulation of the data shown in Table I. Due to an unfortunate error in planning the experiment, not all of the subjects of Part I received the same amount of training before the ninety-day period of no-practice Actually the amount of training varied from nine to fifteen trance periods. The number of trance periods after the period of no-practice was the same for all of the subjects. In order to determine the effect of the no-practice period it was consequently necessary to re-tabulate the data. The last value of each subject was averaged for trial No. 1, the second from the last for trial No. 2, the third from the last for trial No. 3, etc.

TABLE III

THE NUMBER OF SECONDS REQUIRED ON SUCCESSIVE DAYS TO PRODUCE LID CLOSURE BY HYPNOTIC SUGGESTION

Trial Number	Time in Seconds Required for Lid Closure					
	Subject B	Subject D	Subject F	Subject H	Subject S	Mean Time
14.....	100	435	77	274	109	199
13.....	90	372	117	220	94	179
12.....	50	309	149	205	87	160
11.....	50	330	155	156	73	153
10.....	25	229	87	180	99	124
9.....	23	197	130	180	102	126
8.....	23	200	92	160	85	112
7.....	18	185	69	114	135	104
6.....	13	149	74	107	124	93
Ninety-day Interval						
5.....	13	205	90	133	92	107
4.....	10	195	80	120	105	102
3.....	13	185	69	100	115	96
2.....	12	188	80	107	70	91
1.....	10	125	83	135	71	85

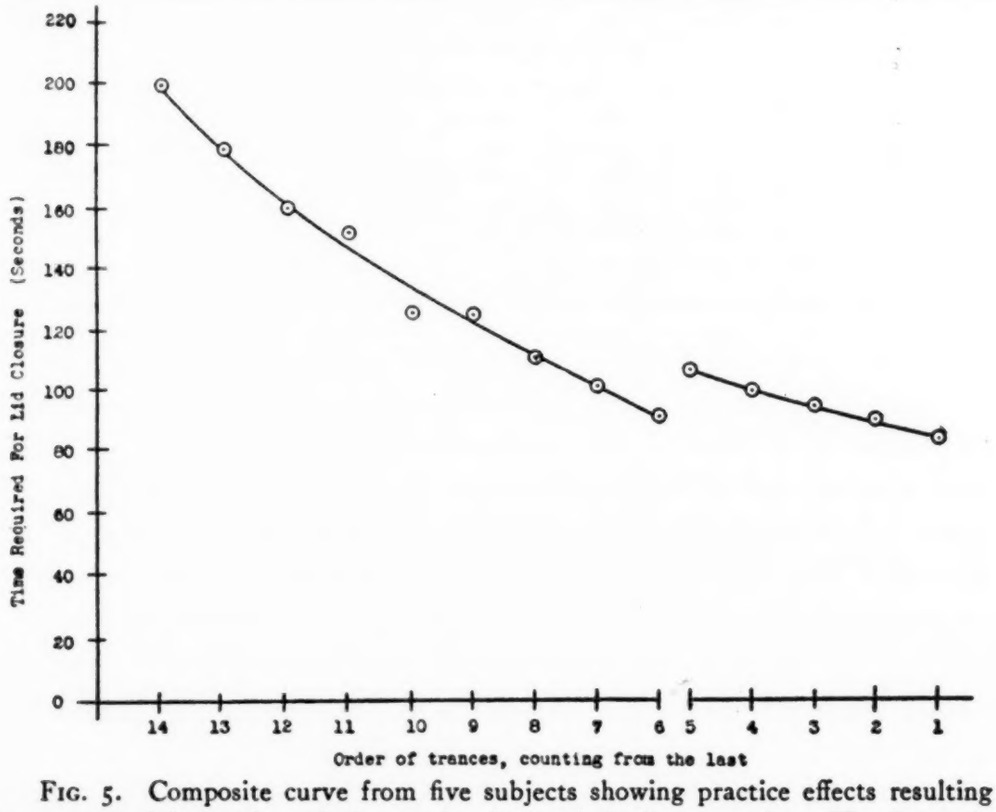

FIG. 5. Composite curve from five subjects showing practice effects resulting from repeated hypnosis and the result of a period of no-practice. The intervals between trances 14–6 and 5 to 1 are at least 24 hours. That between 6 and 5 was approximately 13 weeks.

Figure 5 shows the composite results plotted from Table III. The mean increase in the trance time as a result of the no-practice period was fourteen seconds. In the first series two trance periods were required to cause this decrease; in the second series five trance periods were required to bring about a corresponding change. This shows that the rate of decrease of the trance time for the second series is less than that of the first.

In comparing the losses during the no-practice periods of Parts I and II, it is seen that the loss during the ninety-day period is less than that of the twenty-four hour period. A possible explanation is that retention after a slowly learned series is greater than that of a quickly learned series.

Summary

There is at first a marked decrease in the length of time necessary to induce each successive hypnotic trance. In the course of these trances this falls to a value of about 25 per cent that of the original time.

The rate of decrease becomes less as the practice continues.

There is a loss in the susceptibility to the trance with a period of no-practice.

Upon resuming practice, the length of time required to induce the trance again decreases.

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THE SUMMATION METHOD IN STATISTICS

BY

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Although the summation method for the computation of unadjusted moments has been described by many (Hardy, Elderton, Lidstone, Lipps, Wirth, Czuber, Brown and Thompson, and Levinski), it has found little application among statistical workers.[1] None of the American text-books mention it and only very few of the British and Continental texts treat it at all. Indeed, it is the writers' ignorance of some special forms of this method and their belief of having first discovered it that led to the present experiments in which the great superiority of computing with or without a comptometer, means, standard deviations and Σxy's, by summating around the mean was clearly demonstrated. The details of the experiments were as follows:

The Experiments

(*a*) Fourteen means and standard deviations of 16 entries each, representing frequencies of wages in dollars in a research problem for the Vocational Service for Juniors, were computed by each of the following methods: (1) summating around the mean, using a slight modification of the Lidstone method to be described later, and (2) by the two usual multiplicative methods: of (*a*) either taking zero as the mean and multiplying the frequencies by the squares of the scores or (*b*) guessing the mean near the correct mean and multiplying the frequencies by the deviations and by the squares of the deviations in class-interval values. The class-interval values ran from $5.00 to $20.00; the total frequencies from 99 to 1000; the following being an example: 4, 5, 4, 14, 9, 106, 16, 137, 24, 36,

[1] Warren and Mendenhall with a special device (the Mendenhall-Warren-Hollerith Correlator described in *Columbia University Statistical Bureau Document No. 1*) cumulate to zero, a form found to be quite inefficient for hand calculation in comparison to the method herein described.

31, 10, 4, 4, 1, 4. An electric comptometer was used in the multiplicative methods, but in the summative method it was soon found more efficient not to use it. Two means and two standard deviations were first computed by summation for practice, while the methods of multiplication were already very familiar to the calculator, 100 standard deviations having been computed by these methods prior to the experiment.

The results show that the average time per mean and sigma by the summative method was 3.43 minutes, while for the multiplicative methods it was 8.25 and 6.78 minutes for taking zero as the guessed mean and guessing the mean near the correct mean respectively. The time was taken by means of a stop watch.

(*b*) Five Σxy's of correlation tables of 15–32 x-class-intervals and 5–16 y-class-intervals, with total frequencies from 4,675 to 46,652, and with correlations from .352 to .912 were computed both by the summative method and the usual multiplicative guessed-mean method, the electric comptometer having been used in either case for the addition of the $\Sigma xy'$ columns only. The results show that the average time per Σxy's by the summative method was 12.48 minutes while by the usual multiplicative method it was 15.52 minutes. The greater the scatter and the larger the number of x and y-class-intervals; the greater the saving in computing the $\Sigma x'y$'s by the summative method.

As seen from these two experiments, from one-half to two-thirds of the time of computation of standard deviations and means and about one-fourth of the time for computing Σxy's is saved by the summation method; and, although the comparative efficiency of each method is undoubtedly a function of the number and size of the frequencies, yet the large saving in these rather typical frequency examples is of great significance. The saving will, of course, be still greater in the computation of higher moments around the mean. It should also be noted that the summative method has the further advantage over the multiplicative methods in that it has practically no use for calculators and permits fewer errors, the discrepancies between the results of the two methods being in nearly all cases due to errors in multiplication.

Illustration of the Summative Method

As the method of summation around the guessed mean is not generally known and as the writers' method is slightly different from those described by others and is extended to the computation of Σxy's, the solution of a complete correlation problem and the computation of the third and fourth unadjusted moments around the mean will be given for illustration.

$$\Sigma FD_y = 1235 - 942 = 293$$

$$cy = \frac{293}{679} = .43$$

$$c^2y = .1849$$

$$Cy = .43 \times 5 = 2.15$$

$$\text{G.M.}_{(y)} = 52.5$$

$$\text{Mean}_{(y)} = 52.5 + 2.15 = 54.65$$

$$\Sigma FD_y^2 = 1659 + 2601 + 3584 + 2349 = 10193$$

$$\sigma_y = \sqrt{\frac{10193}{679} - .1849} = 3.85 \times 5.0 = 19.25$$

$$\Sigma FD_y' = 942 + 1235 = 2177$$

$$\text{A.D.} = \frac{2177 + .43(332 - 347)}{679} = 3.20 \times 5.0 = 16.00$$

$$\Sigma FD_y^3 = 6(5435 - 3574) + 293 = 11459$$

$$\Sigma FD_y^4 = 12(3391 + 6965 + 11410 + 5975) + 10193$$
$$= 343085$$

$$\Sigma FD_x = 1289 - 1361 = -72$$

$$c_x = \frac{-72}{679} = -.106$$

$$c_x^2 = .0112$$

$$\Sigma FD_x^2 = 2773 + 4134 + 4211 + 2922 = 14040$$

$$\sigma_y = \sqrt{\frac{14040}{679} - .0112} = 4.54$$

$$r = \frac{\frac{8291}{679} - (.43)(-.106)}{3.85 \times 4.54} = .701$$

TABLE I

To Illustrate the Solution of a Correlation Problem and the Computation of the Third and Fourth Moments Around the Mean by the Method of Summation

	Grade Distribution																							
	3L	3H	4L	4H	5L	5H	6L	6H	7L	7H	8L	8H	I	II	III	IV	Col.	f	S^{I}	S^{II}	S^{III}	S^{IV}	S^{V}	$\Sigma x'$	$\Sigma x'y'$
10	3 3	3	5 2	5	5	5	6 1											6	6	6	6	6	6	32	32
15	5 5	8 3	9 1	9	10 1	10	10											10	16	22	28	34	40	61	93
20	7 7	11 4	19 8	20 1	26 6	27 1	29 2		1	1	1 1							30	46	68	96	130	170	136	229
25	17 17	23 6	31 8	35 4	40 5	41 1	43 2											43	89	157	253	383	553	230	459
30	9 9	12 3	20 8	25 5	30 5	31 1	35 4	4	2	2	2 1	1	1 1					41	130	287	540	923	1476	154	613
35	1 1	5 4	13 8	16 3	25 9	25	29 4		6 2	4 1	3 1	2 2						35	165	452	992	1915	3391	99	712
40	5 5	13 8	18 5	23 5	30 7	33 3	36 3	1	13	13 4	9 7	2 1	1 1					50	215	667	1659	3574	6965	120	832
45	3 3	7 4	12 5	13 1	24 11	31 7	41 10	2	17 3	14 5	9 3	6 4	2	2 1	1 1			60	275	942	2601	—	—	80	912
50	5		4	1	4	5	13	2	6	3	3	5	3	1	2			57	Guessed mean						

Table taken from Otis, Margaret, A study of suggestibility of children, *Archives of Psychol.*, 1924, No. 70.

TABLE I—*Continued*

	Grade Distribution																						
	3L	3H	4L	4H	5L	5H	6L	6H	7L	7H	8L	8H	I	II	III	IV	Col.	*f*	S^{I}	S^{II}	S^{III}	S^{IV}	S^{V}	$\Sigma x'$	$\Sigma x'y'$
55	1 1	4 3	7 3	8 1	18 10	21 3	29 8	8	36 3	33 4	29 7	22 14	8 4	4 1	3 3			73	347	1235	3584	—	—	47	858
60	2 2	4 2	6 2	7 1	11 4	13 2	20 7	3	35 11	24 4	20	20 12	8 3	5 1	4 4			58	274	888	2349	5435	11410	53	811
65			2 2	5 3	8 3	10 2	10	4	42 9	33 5	28 2	26 11	15 8	7 4	3 1	2	2 2	56	216	614	1461	3086	5975	123	758
70		1 1	4 3	5 1	5	8 3	13 5	2	33 7	26 2	24 3	21 9	12 6	6 2	4 3	1	1 1	48	160	398	847	1625	2889	92	635
75				1 1	3 2	4 1	5 1	2	37 7	30 1	29 2	27 9	18 6	12 4	8 3	5 3	2 2	44	112	238	449	778	1264	155	543
80					1 1	1	4 2		28 6	22	22	22 2	20 7	13 5	8 4	4 1	3 3	31	68	126	211	329	486	137	388
85								1	21 3	18	18	18	18 2	16 3	13 5	8 1	7 7	22	37	58	85	118	157	137	251
90								1	8	8	8	8	8 2	6	6	6 1	5 5	9	15	21	27	33	39	63	114
95									6	6	6	6	6	6 1	5	5	5 5	6	6	6	6	6	6	51	51
f	58	38	59	27	68	29	62	30	57	29	30	69	43	23	26	6	25	679							8291
S^{1}	58	96	155	182	250	279	341	Guessed mean	308	251	222	192	123	80	57	31	25								
S^{2}	58	154	309	491	741	1020	1361		1289	981	730	508	316	193	113	56	25								
S^{3}	58	212	521	1012	1753	2773	4134		4211	2922	1941	1211	703	387	194	81	25								

Description of the Calculations

Only calculations which do not occur in the multiplicative methods will be described.

The S-columns.—The entries in the S^1 columns are obtained by cumulating the entires in the *f*-columns both from the top and from the bottom towards, but not including, the guessed mean. Thus, the first entry at the top of the S^1 column is 6; the second is 16, 6 plus 10; the third is 46, 16 plus 30; and so forth. Beginning at the bottom, the last entry in the S^1 column is 6; the second last is 15, 6 plus 9; the third last is 37, 15 plus 22; and so forth. The entries in the S^{II} column are similarly cumulated from the entries in the S^1 column; the entries in the S^{III} column from those in the S^{II}; the entries in S^{IV} from those in S^{III}; and so on. In the S^{IV} and S^V columns, the last entries on either side of the guessed mean are not calculated.

The First Unadjusted Moment around the Mean, and the Correction 'c'.—The first unadjusted moment, ΣFD, is obtained by subtracting in the S^{II} column the entry immediately below the guessed mean from that immediately above [2] the guessed mean. Thus, in this example, for the *y*-variable, it is 1235 − 942, or 293. The correction to the guessed mean is, of course, 293 divided by 679 (N), the number of cases, or .43.

The Calculation of the AD.—The ΣFD without taking account of the signs, or $\Sigma FD'$, is obtained by adding in the S^{II} column the entry immediately above to that immediately below the guessed mean. The entry in the S^1 column immediately above the guessed mean, is the F_o; while the entry in the same column immediately below, plus the frequency of the guessed mean, is the F_l.

The Second Unadjusted Moment around the Mean.—This moment, ΣFD^2, necessary for the calculation of the standard deviation, is obtained by adding in the S^{III} column the two entries immediately above the guessed mean to the two entries immediately below it. Thus, ΣFD^2 is 1659 + 2601 + 3584 + 2349 or 10193.

[2] *Above* and *below* are used in the sense of higher and lower in class-interval value.

The Higher Moments.—The third unadjusted moment around the mean, or ΣFD^3, is obtained by subtracting in the S^{IV} column the entry below the guessed mean from that above the guessed mean, multiplying the difference by 6, and adding the ΣFD to the product. Thus in our example, it is $6(5435 - 3574) + 293$, or 11459. The fourth unadjusted moment, or ΣFD^4, is obtained by adding the two entries above the guessed mean to the two below the guessed mean in column S^V, multiplying this sum by 12, and adding ΣFD^2 to this product; in our example, it is $12(3391 + 6965 + 11410 + 5975) + 10193$, or 343085.

The $\Sigma x'y'$ Column.—From either end of each row in the scatter-diagram, except the row of the guessed mean, the entries are cumulated towards, but not including the column of the guessed mean, each cumulated entry being written above each frequency. Thus, in the first row of our scatter-diagram above the frequency entries, 3, 3, 5, 5, 5, 5, 6 are the cumulated entries of the row 3, 0, 2, 0, 0, 0, 1. The cumulated rows are then added horizontally, account being taken of the sign of the quadrant, and entered under $\Sigma x'$. Thus, the first entry in x' is 32, or $3 + 3 + 5 + 5 + 5 + 5 + 6$ The $\Sigma x'$ entries are then cumulated from the bottom and the top, as previously described, to form the $\Sigma x'y'$.[3] The entries in the $\Sigma x'y'$ column are then totaled to give the Σxy, in our case 8291.

Calculation by Summing the Summative Columns.—It follows from the manner of the construction of summative columns that the sum of x entries in some rth summative column is the same as the xth entry in the next or $r + 1$ summative column, by formula: $\Sigma S_x^r = S_x^{r+1}$. The sums of the columns may, therefore, be used to compute moments, the construction of one summative column being thereby saved. Thus, the first moment, or ΣFD, may be obtained by subtracting in the S^1 column the sum of all the entries below the guessed mean from the sum of all the entries above the

[3] In cumulating $\Sigma x'$ entries, negative and positive values are added algebraically, so that any entry in the $\Sigma x'y'$ column may be positive or negative, and the final Σxy positive or negative.

guessed mean, or by formula: $\Sigma FD = \Sigma S^{I}_{a} - \Sigma S^{I}_{b}$, while the $\Sigma FD'$ for the calculation of the A.D. could be found by adding all the entries in S^{I} column, or by formula: $\Sigma FD' = \Sigma S^{I}_{n}$. Similarly, the second moment would be obtained by adding the first moment to twice the sum of all the entries—not including the entry immediately above the guessed mean—in the S^{II} column, or by formula: $\Sigma FD^2 = \Sigma FD + 2(\Sigma S^{II}_{nb} + \Sigma S^{II}_{(n-1)a})$. However, when the A.D. is also calculated, it would be simpler to compute the second moment by subtracting the sum of all the entires in the S^{I} column, or the $\Sigma FD'$, from twice the sum of all the entries in the S^{II} column, or by formula: $\Sigma FD^2 = 2\Sigma S^{II}_{n} - \Sigma S^{I}_{n}$. In like manner the third moment, ΣFD^3 would result from subtracting in the S^{III} column the sum of all the entries below from the sum of those above the guessed mean, but not including the entries immediately above and immediately below the guessed mean multiplying this difference by 6 and adding to this product ΣFD, or by formula: $\Sigma FD^3 = 6(\Sigma S^{III}_{na} - \Sigma S^{III}_{nb}) + \Sigma FD^2$. The fourth unadjusted moment would be obtained by subtracting in the S^{IV} column the entry below plus the entry above the guessed mean from twice the sum of all the entries in the same column, multiplying this difference by 12, and adding to it FD^2, or by formula: $\Sigma FD^4 = 12\,[2\Sigma S^{IV}_{n} - (S^{IV}_{a} + S^{IV}_{b})] + \Sigma FD^2$.

If only the mean, or A.D., or both are desired, this method of sums is undoubtedly the most time-saving. A 15 percent to 20 percent time-saving was found in calculating means and standard deviations by summing the S^{II} column, multiplying this by 2 and subtracting therefrom the sum of the first entries on either side of the guessed mean in the same S^{II} column.

Proof

Although proofs for the computation of moments by the summation method have been given previously by Hardy, Elderton, and Czuber, the writers still present their proof, worked out independently, for the following reasons: (1) It relates the Method of Summation to Pascal's arithmetical Triangle and offers a general formula whereby, if only the

frequency column is known, any entry in any column and the sum of any column may be computed without the computation of intermediate entries. (2) To follow it requires less mathematical knowledge.

General Formula for Summative Columns in Terms of Class-interval Frequencies.—The general formula for summative columns is really the same as that for figurative numbers in Pascal's Triangle. Indeed, Pascal constructed the numbers in his triangle by making each number the sum of that immediately above it and that immediately to the left of it, or summating them. Unlike the first series in figurative numbers, however, the frequencies are neither equal to one nor as a rule to one another, and therefore, the general formula will take the following form:

General Formula for Summative Columns in Terms of Class-interval Frequencies.—Let n denote the total number of class-intervals above or below the G.M. or any other vertical; x any number of class-intervals; $f_n, f_{n-1}, f_{n-2}, \ldots f_3, f_2, f_1$ the frequencies $n, n-1, n-2 \ldots 3, 2$, and 1 class-interval above or below the G.M. or any other vertical.[4] Let further $\Sigma S^{I}f(n)$, $\Sigma S^{II}f(n)$, $\Sigma S^{III}f(n) \ldots \Sigma S^{r}f(n)$ denote respectively the sum of n entries in the first, the second, the third . . . the rth summative column; $S^{r}f(n-x)$ the sum of $n-x$ entries in some rth summative column; $S^{r}1a$, $S^{r}xa$ the entry in some rth summative column respectively 1, x class-intervals above the G.M. Then, $\Sigma S^{r}f(n)$ will equal to ${}_{n+r-1}C_r f_n + {}_{n+r-2}C_r f_{n-1} + {}_{n+r-3}C_r f_{n-2} + \ldots + {}_{n+r-(n-2)}C_r f_3 + {}_{n+r-(n-1)}C_r f_2 + {}_{n+r-n}C_r f_1$ and $\Sigma S^{r}f(n-x)$ to ${}_{n+r-1-x}C_r f_n + {}_{n+r-2-x}C_r f_{n-1} + {}_{n+r-3-x}C_r f_{n-2} + \ldots + {}_{n+r-(n-2)}C_r f_{x+3} + {}_{n+r-(n-1)}C_r f_{x+2} + {}_{n+r-n}C_r f_{x+1}$, the denominators of the expanded series of combinations being in both expressions constant and equal to factorial r, while the numerators of the series form an arithmetical progression of (-1) as the common difference and of n terms and $n+r-1$ as the first term in $\Sigma S^{r}f(n)$ and $n-x$ terms and $n+r-1-x$ as the first term in $\Sigma S^{r}f(n-x)$. Similarly, S_r1a will equal to ${}_{n+r-2}C_{r-1} f_n + {}_{n+r-3}C_{r-1} f_{n-1} + {}_{n+r-4}C_{r-1} f_{n-2} + \ldots + {}_{n+r-(n-1)}C_{r-1} f_2$

[4] 'Above or below the g.m. or any other vertical' is understood whenever only 'above the g.m.' is written.

$+ {}_{n+r-n}C_{r-1}f_2 + {}_{n+r-(n+1)}C_{r-1}f_1$ and $S^r xa$ to ${}_{n+r-x-1}C_{r-1}f_n + {}_{n+r-x-2}C_{r-1}f_{n-1} + {}_{n+r-x-3}C_{r-1}f_{n-2} + \ldots + {}_{n+r-(n-1)}C_{r-1}f_{x+3} + {}_{n+r-n}C_{r-1}f_{x+2} + {}_{n+r-(n+1)}C_{r-1}f_{x+1}$, the denominators of the expanded series of combinations being here equal in both expressions to factorial $r1$, while the numerators form an A.P. of (-1) as the common difference and of n terms and $n + r - 2$ as the first term in $S^r 1a$ and of $n - x$ terms and $n + r - x - 1$ as the first term in $S^r xa$.

The correctness of this general formula becomes evident when Table II, in which the entries in the first five summative columns are given in terms of class-interval frequencies, is examined. The coefficients of the frequencies in S^{I} and all equal to one, while in ΣS^{I} they form an A.P. of the first n natural numbers. In S^{II} the coefficients are A.P.'s of the first n, $n - 1$, $n - 2 \ldots 3, 2, 1$ natural numbers in the rows respectively $1, 2, 3 \ldots n - 2, n - 1, n$ class-intervals above the G.M., while in ΣS^{II} the coefficients are a series of sums of A.P.'s of the first n, $n - 1$, $n - 2 \ldots 3, 2, 1$ natural numbers forming an A.P. of the second order. The coefficients in S^{III} are A.P.'s of the same order as those in S^{II}, while another order of A.P.'s is added in ΣS^{III}. Similarly, for S^{IV}, ΣS^{IV}, S^{V}, and ΣS^{V}, another order of A.P.'s being added with the sum of each new summative column. In general, the coefficients of the frequencies in summative columns are a succession of series in which the sum of n or $n - x$ terms in one column become respectively the nth or the $n - x$th term in the next succeeding column. Now, if the formulas in each column of Table II, easily obtained by any method of summation of series, are further examined, the following will be noted. In the sums of the columns, the denominators of the coefficients of the various frequencies always equal to factorial r, r being the number of the summative column, while the numerators form A.P.'s of n terms and $n + r - 1$ as the first terms and with (-1) as the common difference. In the entries of the other rows, the denominators are always equal to factorial $r - 1$, while the numerators form A.P.'s of n terms and $n + r - 2$ as the first terms in the rows immediately above the G.M. and of $n + 1 - x$ terms and $n + r - 1 - x$

TABLE II

ENTRIES IN SUMMATIVE COLUMNS IN TERMS OF CLASS-INTERVAL FREQUENCIES

	S-Columns / *F*-Column	S^{I}	S^{II}	S^{III}	S^{IV}	S^{V}
	f_n	f_n	f_n	f_n	f_n	f_n
	f_{n-1}	$f_n + f_{n-1}$	$2f_n + f_{n-1}$	$3f_n + f_{n-1}$	$4f_n + f_{n-1}$	$5f_n + f_{n-1}$
	f_{n-2}	$f_n + f_{n-1} + f_{n-2}$	$3f_n + 2f_{n-1} + f_{n-2}$	$6f_n + 3f_{n-1} + f_{n-2}$	$10f_n + 4f_{n-1} + f_{n-2}$	$15f_n + 5f_{n-1} + f_{n-2}$
	f_3	$f_n + f_{n-1} + f_{n-2} + \cdots f_3$	$(n-2)f_n + (n-3)f_{n-1} + (n-4)f_{n-2} + \cdots + f_3$	$\frac{(n-1)(n-2)}{\lfloor 2} f_n + \frac{(n-2)(n-3)}{\lfloor 2} f_{n-1} + \frac{(n-3)(n-4)}{\lfloor 2} f_{n-2} + \cdots + f_3$	$\frac{(n)(n-1)(n-2)}{\lfloor 3} f_n + \frac{(n-1)(n-2)(n-3)}{\lfloor 3} f_{n-1} + \frac{(n-2)(n-3)(n-4)}{\lfloor 3} f_{n-2} + \cdots + f_3$	$\frac{(n+1)(n)(n-1)(n-2)}{\lfloor 4} f_n + \frac{(n)(n-1)(n-2)(n-3)}{\lfloor 4} f_{n-1} + \frac{(n-1)(n-2)(n-3)(n-4)}{\lfloor 4} f_{n-2} + \cdots + f_3$
	f_2	$f_n + f_{n-1} + f_{n-2} + \cdots f_3 + f_2$	$(n-1)f_n + (n-2)f_{n-1} + (n-3)f_{n-2} + \cdots 2f_3 + f_2$	$\frac{(n)(n-1)}{\lfloor 2} f_n + \frac{(n-1)(n-2)}{\lfloor 2} f_{n-1} + \frac{(n-2)(n-3)}{\lfloor 2} f_{n-2} + \cdots + 3f_3 + f_2$	$\frac{(n+1)(n)(n-1)}{\lfloor 3} f_n + \frac{(n)(n+1)(n+2)}{\lfloor 3} f_{n-1} + \frac{(n-1)(n-2)(n-3)}{\lfloor 3} f_{n-2} + \cdots + 4f_3 + f_2$	$\frac{(n+2)(n+1)(n)(n-1)}{\lfloor 4} f_n + \frac{(n+1)(n)(n-1)(n-2)}{\lfloor 4} f_{n-1} + \frac{(n)(n-1)(n-2)(n-3)}{\lfloor 4} f_{n-2} + \cdots + 5f_3 + f_2$
	f_1	$f_n + f_{n-1} + f_{n-2} + \cdots + f_3 + f_2 + f_1$	$nf_n + (n-1)f_{n-1} + (n-2)f_{n-2} + \cdots + 3f_3 + 2f_2 + f_1$	$\frac{(n+1)(n)}{\lfloor 2} f_n + \frac{(n)(n-1)}{\lfloor 2} f_{n-1} + \cdots + \frac{(n-1)(n-2)}{\lfloor 2} f_{n-2} + \cdots + 6f_3 + 3f_2 + f_1$	$\frac{(n+2)(n+1)(n)}{\lfloor 3} f_n + \frac{(n+1)(n)(n+1)}{\lfloor 3} f_{n-1} + \frac{(n)(n+1)(n-2)}{\lfloor 3} f_{n-2} + \cdots + 10f_3 + 4f_2 + f_1$	$\frac{(n+3)(n+2)(n+1)(n)}{\lfloor 4} f_n + \frac{(n+2)(n+1)(n)(n-1)}{\lfloor 4} f_{n-1} + \frac{(n+1)(n)(n-1)(n-2)}{\lfloor 4} f_{n-2} + \cdots + 15f_3 + 5f_2 + f_1$
Σ-Row	$f_n + f_{n-1} + f_{n-2} + \cdots f_3 + f_2 + f_1$	$nf_n + (n-1)f_{n-1} + (n-2)f_{n-2} + \cdots + 3f_3 + 2f_2 + f_1$	$\frac{(n+1)(n)}{\lfloor 2} f_n + \frac{(n-1)(n-2)}{\lfloor 2} f_{n-1} + \cdots + 6f_3 + 3f_2 + f_1$	$\frac{(n+2)(n+1)(n)}{\lfloor 3} f_n + \frac{(n+1)(n)(n-1)}{\lfloor 3} f_{n-1} + \frac{(n)(n-1)(n-1)}{\lfloor 3} f_{n-2} + \cdots + 10f_3 + 4f_2 + f_1$	$\frac{(n+3)(n+2)(n+1)(n)}{\lfloor 4} f_n + \frac{(n+2)(n+1)(n)(n-1)}{\lfloor 4} f_{n-1} + \frac{(n+1)(n)(n-1)(n-2)}{\lfloor 4} f_{n-2} + \cdots + 15f_3 + 5f_2 + f_1$	$\frac{(n+4)(n+3)(n+2)(n+1)(n)}{\lfloor 5} f_n + \frac{(n+3)(n+2)(n+1)(n)(n-1)}{\lfloor 5} f_{n-1} + \frac{(n+2)(n+1)(n)(n-1)(n-2)}{\lfloor 5} f_{n-2} + \cdots + 21f_3 + 15f_2 + f_1$

as the first terms in the rows x class-intervals above the G.M. All this is what is given in the preceding paragraph by the general formulas for summative columns in terms of series of combinations of coefficients of class-interval frequencies.

Proof of the Identity of the Summative and the Multiplicative Methods.—The First Moment.—The first unadjusted moment around the mean is obtained in the summative method by subtracting in the S^{II} column the entry immediately below from that of immediately above the G.M. The subtraction is merely to obviate the need of attaching negative signs to the entries below the G.M., while in general it is of course sufficient to prove that the entry immediately above the G.M. is identical with the ΣFD above the G.M., the same proof holding for entries immediately below the G.M. or any other vertical. Now, from Table II it is seen that $S^{II}1a$, or the entry immediately above the G.M., in the S^{II} column, equals to $nf_n + (n-1)f_{n-1} + (n-2)f_{n-2} + \ldots + 3f_3 + 2f_2 + f_1$, but n, $n-1$, $n-2 \ldots 3$, 2, 1 are the number of class-intervals which the corresponding frequencies $f_n, f_{n-1}, f_{n-2} \ldots f_3, f_2, f_1$ are above the G.M., and the whole expression may thus be rewritten as $f_nD_n + f_{n-1}D_{n-1} + f_{n-2}D_{n-2} + \ldots + f_3D_3 + f_2D_2 + f_1D_1$, or $S^{II}1a$ is identical with ΣFD above the G.M.

The Second Moment.—This moment is found by adding in the S^{III} column the two entries immediately above with the two immediately below the G.M. Here, too, the proof need be given only that the sum of the two entries immediately above the G.M. is identical with ΣFD^2 above the G.M. From Table II it may be seen that the sum of these two entries, or $S^{III}2a + S^{III}1a$, equals to

$$\left[\frac{n(n-1)}{2!}f_n + \frac{(n-1)(n-2)}{2!}f_{n-1} + \frac{(n-2)(n-3)}{2!}f_{n-2} + \ldots + 3f_3 + f_2\right] + \left[\frac{(n+1)(n)}{2!}f_n + \frac{n(n+1)}{2!}f_{n-1} + \frac{(n-1)(n-2)}{2!}f_{n-2} + \ldots + 6f_3 + 3f_2 + f_1\right]$$

Adding up the two series and simplifying, the expression

becomes $n^2f_n + (n-1)^2f_{n-1} + (n-2)^2f_{n-2} + \ldots + 9f_3 + 4f_2 + f_1$, which may be rewritten as $f_nD_n^2 + f_{n-1}D_{n-1}^2 + f_{n-2}D_{n-2}^2 + \ldots + f_33^2 + f_22^2 + f_1$, showing that $S^{III}2a + S^{III}1a$ is identical with ΣFD^2 above the G.M.

The Third Moment.—This unadjusted moment is obtained in the summative method by subtracting in the S^{IV} column the last calculated entry below the G.M. from the last calculated entry above the G.M., multiplying the difference by 6, and adding to the product the first unadjusted moment. Again, we need only prove that 6 times the last calculated entry above the G.M. in the S^{IV} column plus the entry immediately above the G.M. in the S^{II} column is identical with FD_3 above the G.M. According to Table II, these entries, $6(S^{IV}2a)S^{II}1a$, are equal to

$$6\left[\frac{(n+1)n(n-1)}{3!}f_n + \frac{n(n-1)(n-2)}{3!}f_{n-1} + \frac{(n-1)(n-2)(n-3)}{3!}f_{n-2} + \ldots + 4f_3 + f_1\right] + \left[nf_n + (n-1)f_{n-1} + (n-2)f_{n-2} + \ldots + 3f_3 + 2f_2 + f_1\right].$$

Adding up and simplifying, the expression becomes $n^3f_n + (n-1)^3f_{n-1} + (n-2)^3f_{n-2} + \ldots + 27f_3 + 8f_2 + f_1$, which may be rewritten as $f_nD_n^3 + f_{n-1}D_{n-1}^3 + f_{n-2}D_{n-2}^3 + \ldots + f_33^3 + f_22^3 + f_1$, or $6(S^{IV}2a) + S^{II}1a$ is identical with ΣFD^3 above the G.M.

The Fourth Moment.—Using only the entries above the G.M., we have to prove that 12 times the sum of the two entries above the G.M. in the S^{V} column plus the sum of the two entries immediately above the G.M. in the S^{II} column is identical with ΣFD^4 above the G.M. From Table II we learn that in terms of frequencies this equals to

$$12\left\{\left[\frac{(n+2)(n+1)n(n-1)}{4!}f_n + \frac{(n+1)n(n-1)(n-2)}{4!}f_{n-1} + \frac{n(n-1)(n-2)(n-3)}{4!}f_{n-2} + \cdots + 5f_3 + f_2\right] + \left[\frac{(n+1)n(n-1)(n-2)}{4!}f_n + \frac{n(n-1)(n-2)(n-3)}{4!}f_{n-1} + \frac{(n-1)(n-2)(n-3)(n-4)}{4!}f_{n-2} + \cdots + f_3\right]\right\} + \left[\frac{(n+1)n}{2!}f_n + \frac{n(n-1)}{2!}f_{n-1} + \frac{(n-1)(n-2)}{2!}f_{n-2} + \cdots + 6f_3 + 3f_2 + f_1\right] + \left[\frac{n(n-1)}{2!}f_n + \frac{(n-1)(n-2)}{2!}f_{n-1} + \frac{(n-2)(n-3)}{2!}f_{n-2} + \cdots + 3f_3 + f_2\right].$$

Adding, multiplying, and simplifying, the expression becomes: $n^4f_n + (n-1)^4f_{n-1} + (n-2)^4f_{n-2} + \ldots + 81f_3 + 16f_2 + f_1$, which may be rewritten as $f_nD_n{}^4 + f_{n-1}D_{n-1}{}^4 + f_{n-2}D_{n-2}{}^4 + \ldots + f_33^4 + f_22^4 + f_1$, or the values obtained from $12(S^v3a + S^v2a) + (S^{III}2a + S^{III}1a)$, the method of calculating the fourth moment by summation, are identical with those obtained from FD^4 in the method of multiplication.

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MEMORY FOR THE PLEASANT AS COMPARED WITH THE UNPLEASANT

BY ARTHUR JERSILD

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Do we forget the unpleasant events of life more readily than the pleasant? This question is of interest from many points of view. It raises the general philosophical inquiry as to whether life is predominantly agreeable or disagreeable. It reminds of the speculations of the psychoanalysts who speak of the suppression of painful experiences. It has definite implications for pedagogy and the theory of the discipline of children.

Many studies have been reported bearing upon the problem here set forth. The results of early investigations led to the generalization that unpleasant experiences fade from memory more quickly than the pleasant. This principle was given expression in such terms as 'the optimism of memory', and the 'obliviscence of the disagreeable'.

In testing the question here involved a very significant control must be observed. The mere classification of immediate memories, both for names and events, may yield a predominance of pleasant items. But this procedure does not give an adequate measure of relative amount of forgetting that has taken place for agreeable and disagreeable items. There may have been a numerical preponderance of pleasant events in the past experiences from which these memories are drawn.[1] In testing the problem it is therefore necessary

[1] In a study of the feelings and emotions of normal subjects in everyday life, J. C. Flügel (*Brit. Jour. Psych.*, 1925, 15, 318–355) found a decided predominance of pleasant over unpleasant experiences in the diaries of each of nine subjects who kept a record for a period of thirty days. A. Wohlgemuth (The Influence of Feeling on Memory, *Brit. Jour. Psychol.*, 1922–23, 13, 405–409) found that school children reported a greater number of pleasures than unpleasures when asked to record their experiences of an immediately preceding vacation period. On a later test for recall, there was evidence of greater memory for pleasures.

to take account of the relative number of pleasant and unpleasant items in the original experiences which are subject to recall.

The present study was designed to measure the forgetting of affective experiences over a period of time, with attention directed mainly upon the relative number of agreeable and disagreeable events forgotten. A description of the procedure follows.[2]

Fifty-one students, all of them members of a psychology class, served as subjects in the experiment. On a given day, during the regular class hour, the group was told to record, as rapidly and as comprehensively as possible, all the pleasant experiences which they had had during the preceding three weeks. After a seven minute interval the papers were collected. A second set of sheets was distributed, and this time the subjects were told to record all the unpleasant experiences that had occurred during the preceding three weeks. A seven minute period was again allowed, after which the papers were collected, and nothing more was said.

After the elapse of twenty-one days a second test was applied. The subjects were told to record once more as many pleasant experiences belonging to the three-week period in question as they could recall. Seven minutes were again allowed. Following this they were told to record all the unpleasant experiences which they could recall.

This procedure provided the data on which the present study is based. With the materials from both tests in hand, a record could be made of the number of pleasant and unpleasant events first reported and of the events correctly recalled on the second test.

Results

The table below gives the totals, averages, and standard deviations for the original test and the test for recall. It gives also the 'percentage' averages, representing the averages obtained when the recall score for each subject on each test

[2] A review of the literature bearing upon the present problem, including a review of Wohlgemuth's study, is given by H. Meltzer (The Present Status of Experimental Studies on the Relationship of Feeling to Memory, *Psychol. Rev.*, 1930, 37, 124–139).

was calculated as a percentage of his record on the initial report. (Thus, a subject reports 20 items on the initial test, 10 on the subsequent test for recall: his 'percentage' score is 50.) The distributions from which the 'percent recalled' averages were obtained yielded also standard deviation values. These standard deviations were comparable for the pleasure and unpleasures series, and provided a means of determining the statistical reliability of the difference between these averages.

TABLE 1

	Pleasures	Unpleasures
	Initial Test	
Total	834	698
Average	16.35(±6.74)	13.7(±5.3)
	Recall Test	
Total	355	197
Average	7(±3.58)	3.86(±2.14)
	Percent Recalled	
Average	45.69±(19.4)	31.67(±20.4)

Excess of Percent pleasure over unpleasure recalled 14.02
S.D. of difference between p. and u 3.94
Difference/S.D. diff. = greater than 3 (reliable)

The above table offers certain definite findings. First, it is clearly indicated that the group showed a predominance of pleasant over unpleasant items when called upon to report their affective experiences.

Secondly, and more pertinent to our present inquiry, it appears that on a test for recall after the elapse of some time, there is greater memory for the pleasant than for the unpleasant. This holds true not only with regard to the absolute number of items recalled, but also, and more significantly, with regard to the relative number of pleasant items reproduced. When the discrepancy between the initial totals for pleasantness and unpleasantness is ruled out by means of the method of scoring recall in terms of percentages there is statistically reliable evidence to show that the unpleasant is forgotten more readily than the pleasant.

These results are definite and speak for themselves. The further question follows: why do we forget the disagreeable events of our experience more readily than the agreeable? The data represented above do not provide an empirical an-

swer to this question. But some explanations suggest themselves.

In the opinion of the writer, the explanation of this phenomenon is not to be found in a theory of active suppression of painful memories, or in the hypothesis that nature has provided mankind with a special aptitude for forgetting the disagreeable. To be sure, the hypothesis of active suppression, however open it may be to numerous theoretical objections, might be invoked to account for the forgetting of some events. This would hold true more readily in connection with individuals suffering from partial amnesias or from neurotic disorders. In accounting for the forgetting that takes place with normal individuals, on the other hand, the principles at work are much simpler than the assumptions involved in the view that disagreeable experiences are subjected to active suppression. We forget the unpleasant in many cases simply because the event no longer is unpleasant. A painful event serves as a motive for palliative measures. Steps are taken to cope with the condition that gives rise to the disagreeable affective state. The activity pattern of which the unpleasantness was originally a part frequently terminates in alleviation or relief. It may even terminate in a definitely pleasant state, since the successful coping with a difficulty carries an agreeable affective tone.[3] The total event no longer stands out in memory as a painful experience but falls in line with other events on the normal forgetting curve.

The above principle may be re-stated as follows: the affective intensity of many unpleasant events will be reduced or alleviated by remedial influences which form a part of the total configuration to which the unpleasant state belonged. The result will be that on subsequent redintegration of the event there will be no disagreeable accompaniments. According to this view, even if the associations connected with the experience were re-aroused by means of aided recall (such, for

[3] An incidental observation may be made with regard to this point. After the recall test had been given, three subjects volunteered the information that items which had been included on the initial unpleasantness list, and which now were reproduced on that list in accordance with the directions, no longer had an unpleasant tone, but actually had a distinctly pleasant quality.

instance, as the method of prompting or a recognition test), the event would no longer be classed as an unpleasant memory. This does not mean that the alleviatory activities stamp out and cancel the painful affects which set them going. It merely means that the affective value of an event of the past will be modified in terms of the feeling pattern which characterized the situation as a whole. Where pleasures are concerned, on the other hand, no such modifying influences will be at work. Pleasures are also, to be sure, subject to the normal laws of forgetting. But if the situation with which the pleasant state was associated is recalled, the memory thus reported will be more likely to retain the pleasant label.

An additional factor operates in favor of the greater recall of pleasant events. Many unpleasantnesses are forgotten simply through lack of rehearsal. Under ordinary circumstances, pleasant associations will be given more exercise than unpleasant ones. When an individual takes time for retrospection he has a wide range of experiences from which to choose. In relating events of his past to others, in reflecting upon his past for his own amusement, he will choose to recount past successes and joys rather than embarrassments, pains, and defeats. He steers clear of the unpleasant in retrospect much as he steers clear of a rough road or a noxious odor in his everyday life. It is not a case of suppression of the unpleasant so much as merely a matter of avoidance. The result is that pleasant associations are strengthened through rehearsal, while unpleasant associations wane through disuse.

We have stressed two major tenets in explanation of the phenomenon of the greater relative forgetfulness for the unpleasant as compared with the pleasant, namely, the principle that the memory of many unpleasant events will be modified in terms of activities undertaken to relieve the unpleasantness, and, secondly, the principle that agreeable events will be more subject to review and exercise than the disagreeable. Further minor assumptions might be added, but these will suggest themselves as corollary or subsidiary to the above-mentioned two.

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THE EFFECT OF STUTTERING ON VOLUNTARY MOVEMENT

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During the last thirty years a great many workers have repeatedly recorded the aberative and dysintegrative functioning of the speech and breathing mechanism during stuttering. Notable among these are Halle (1), Ten Cate (2) Gutzman (3), Fletcher (4), Travis (5), and Fossler (6). Few workers, however, have studied the effects of stuttering upon neuromuscular mechanisms not directly related to the function of speech. Recently Goett (7), Dræsche (8), and Gustmann and Schilder (9) have called attention to the important neurological significance of automatic associated movements in speech disturbances of normal and neuro-pathological cases. Orton and Travis (10) and Travis and Herren (11) have shown how stutterers differ from normals in activities unrelated either to any specific act of speech or to speech in general. Travis and Fagan (12) have indicated how stutterers differ from normals during speech in reflex activity.

The present study is an outgrowth of the latter studies and attempts to determine the extent and the nature of disturbances during stuttering, in the voluntary activity of some muscle groups not directly concerned with the production of speech.

It was repeatedly observed, in the course of experimental work, that during a stuttering period subjects could not make the correct muscular response to an agreed signal. Apparently voluntary movement was suspended or in some way interfered with by the act of stuttering. The following experimentation was undertaken to further demonstrate and perhaps explain this phenomenon.

Apparatus and Method

Identical rubber bulbs were connected by pieces of rubber tubing of equal size and length to tambours which registered

upon a kymograph drum. A Jaquet chronograph recorded time in intervals of one second. Periods of overt stuttering were designated by use of a magnetic signal marker.

All subjects were male university students between the ages of 20 and 25. There were twelve normal speakers and twelve stutterers. In the stutterers there was a considerable range in the severity of the defect extending from mild clonic to severe tonic speech blocks.

In general a subject was instructed to grasp a bulb in each hand and to alternately flex and extend, in phase, the fingers of the two hands. Records were also taken from each hand separately. No definite rate or strength of movement was required, each subject choosing his own. One restriction, however, was placed upon the rate. It had to be fast enough to be 'interfered with' by transitory speech blockages. In only one instance was the experimentor obliged to enforce this restriction.

For the normals, an experiment consisted of obtaining records during silence and during speech; for the stutterer it consisted of obtaining records during silence, during normal speech and during stuttering.

Daily newspapers were used as reading material and each subject selected at random his own passages. Each subject had a five minute resting period between two minute recording periods.

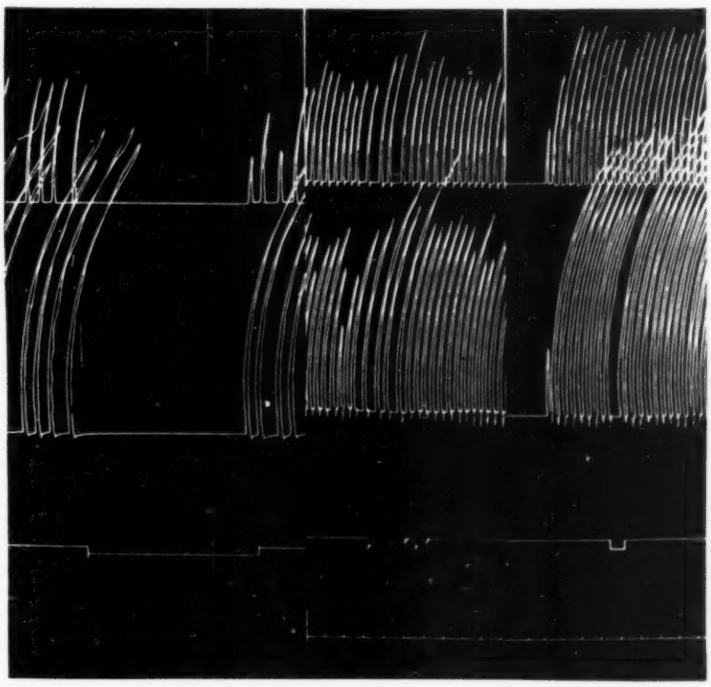
Results

It should be recalled that there are two types of spasms in speech blockages. One, the tonic type, is a generalized, long sustained spasm involving all or a major portion of the entire speech mechanism. In one subject one such spasm lasted nineteen seconds. The other type, the clonic spasm, is a short, transitory, repetitious block. Subjects presenting this type made quick, repetitions of sounds, had lip and throat tremors and presented certain associated movements. It was this latter type of spasm which required a restriction on the minimum rate of voluntary movements.

All of the findings to be reported in the following paragraphs were characteristic of the stutterer only, during at-

tempts at speech. Free speech of the normal speaker had no determinable effect upon voluntary movement as it is measured by our procedure. During silence no differences between the normal and the stutterer were observed.

Among the variations in the rate and extent of movement found in the stutterer's records during stuttering were periods in which there was no movement at all (fig. 1, *A*). The longest of such periods was twenty-two seconds which occurred during a prolonged tonic spasm. Clonic seizures also presented complete inhibition of movement. However, inasmuch as clonic seizures are short, the loss of movement amounted to only one or two beats in the rhythm (fig. 1, *B*) or to a slight delay in initiating the individual movements.



A *B* *C*

FIG. 1. Types of complete cessations of movement.

A. Complete block in the movements of the two hands in a stutterer who presented severe tonic speech spasms. Continuous stuttering.

B. Complete blocks in the movements of the two hands in a stutterer presenting clonic speech spasms.

C. Block in the movement of one hand while the other carries on the rhythm. Clonic stuttering accompanied this block.

Unless otherwise stated in all records the curves from top to bottom are left hand, right hand, signal and time. Down stroke of signal line indicates stuttering.

The usual picture, when both hands were used, was for the loss of movement to occur in the two hands at the same time although one hand sometimes continued to carry on the rhythm (fig. 1, *C*).

Another variation was a gradual decrease in the extent of movements corresponding roughly to the length and severity of the speech block (fig. 2). This type was, of course, most frequently encountered in records made during a tonic speech block. These periods of increased tension were often followed by a complete loss of movements.

FIG. 2. A gradual decrease in the extent of movements roughly corresponding to the severity of the speech block. Continuous stuttering.

Very wide differences in extent occurred in adjacent movements in all of the stutterer's records. These may be due to blocks being suddenly released and immediately imposed again. It was frequently noted that with the 'blurting out' of a sound on which stuttering had occurred, a movement more vigorous than either the one immediately preceding it or the one just succeeding it, when stuttering was again resumed, was registered in both hands (fig. 3).

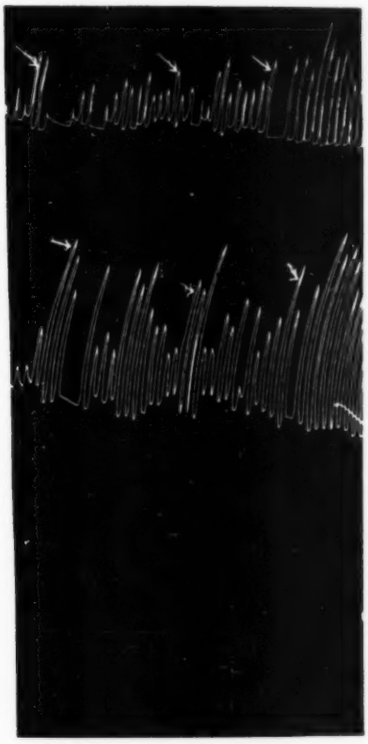
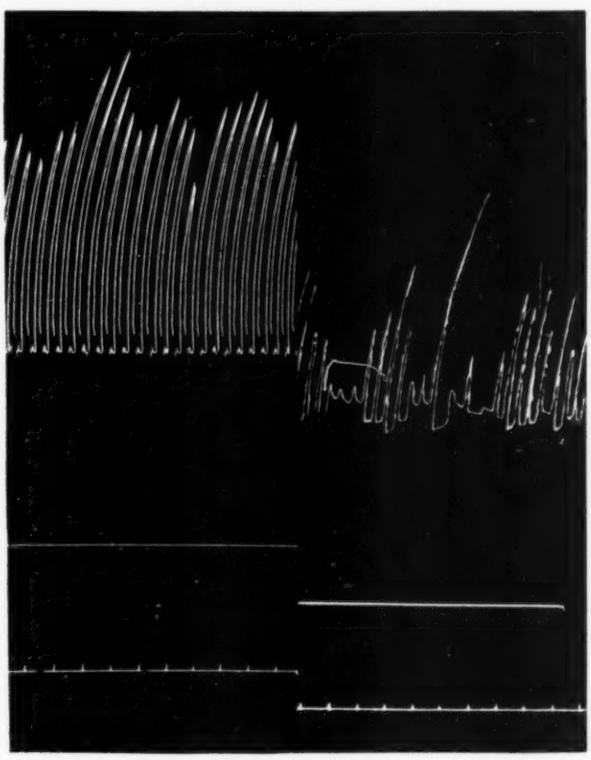


FIG. 3. Arrows designate movements recorded simultaneously with the 'blurting out' of a sound. Continuous stuttering.



A *B*

FIG. 4. Records of the movement of only one hand on the same subject.
A. Right hand during silence.
B. Right hand during stuttering. Continuous stuttering.

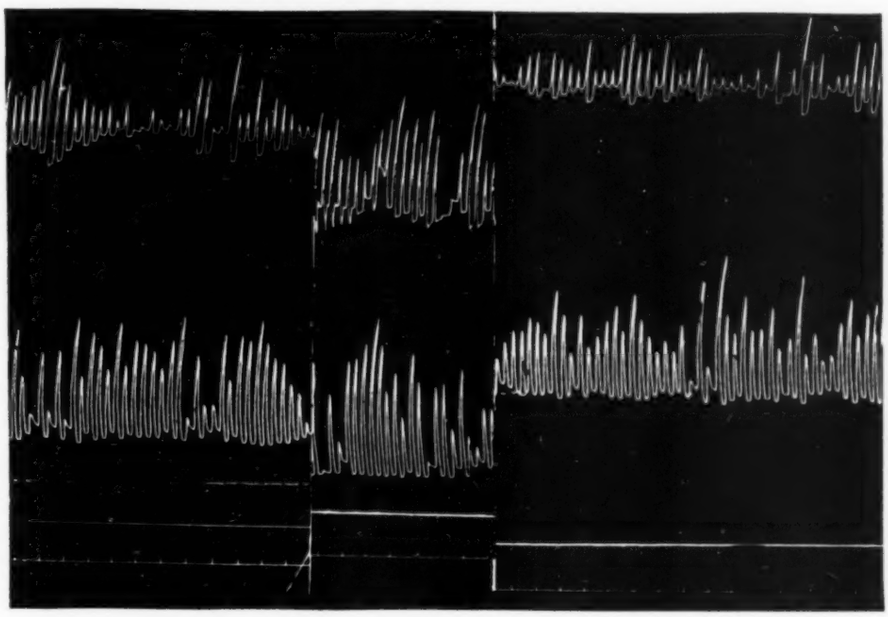
Records of the movements of only one hand during stuttering showed the same alternations in rate and extent as records of the two hands. Furthermore it seemed to make no difference which hand was used as interferances did not appear more often in one hand than in the other (fig. 4, *A*, *B*).

Normally the two hands closed and opened in phase. During stuttering, however, often they moved in a relationship of point-to-point opposition (fig. 5). In continuous

FIG. 5. A point to point opposition in the movements of the two hands. The kymograph drum was revolved at a high rate of speed for this record. Continuous stuttering.

movements of the sort reported in this study, there is for both normals and stutterers a periodic fluctuation in the extent of the movements which are here called waves (fig. 6). For the normal speaker the waves of the two hands were in phase, never varying more than two individual movements. For the stutterer these waves showed the following abnormalities: (1)

FIG. 6. Waves in the movements of the two hands of a normal speaker.



A *B* *C*

FIG. 7. Phase abnormalities in waves of movements.

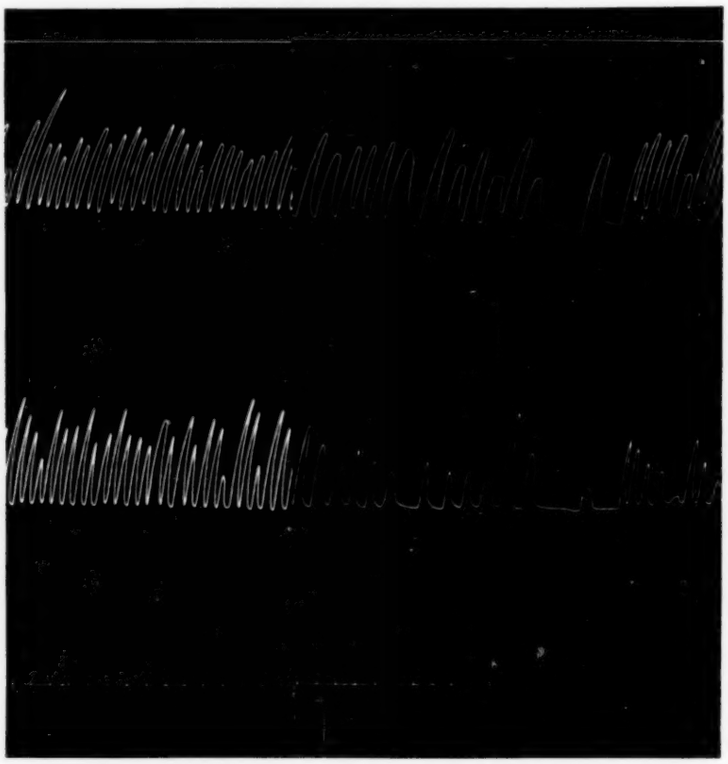
A. Direct opposition of waves in the two hands. Continuous stuttering.

B. The wave of the left hand movements is a fraction behind that of the right hand. Continuous stuttering.

C. No waves in the movements of the left hand to correspond to those in the right hand. Continuous stuttering.

direct phase opposition in the two hands; (2) one wave a fraction ahead or behind the corresponding one from the other hand; and (3) several waves in the record of one hand corresponding to no definite ones in the record of the other hand (fig. 7, A, B, C). The rate of these waves when presented is always the same in the two groups of subjects.

Because of a presumably close relationship between the speech and hand centers in the brain that might account for the parallelism between blocks in the movements of the hands and those in the speech mechanism, other members whose neurological representations were more remotely removed from the speech centers were subjected to the same experimentation.



A *B*

FIG. 8. Flexion and extension movements of the toes of the same subject.
A. During silence.
B. During stuttering. Continuous stuttering. The signal line is at the top in both records.

Figures 8*A* and 8*B* show records of alternate flexion and extension of the toes of a stutterer during silence and stutter-

ing. Allowing for a greater degree of regularity that results from the greater skill of the hand there were no results obtained in alternate closing and opening of the hands that did not also appear in alternate flexion and extension of the toes.

Summary

Stuttering speech produces the following effects on voluntarily initiated flexion-extension movements of the hands and feet:

1. Complete inhibition of movement.
2. Reduction in the extent of movement corresponding roughly to the length and severity of stuttering.
3. An occasional vigorous movement accompanying a 'blurting out' of a sound.

The above disturbances may appear in both hands, in only one hand when both are being used or in either hand when it is being used singly. The following observations can be noted only when the two hands are used together:

1. A point to point opposition of the movements of the two hands.
2. Complete or partial opposition in phase of the periodic fluctuations in extent of movement of the two hands.
3. Absence of periodic fluctuations in extent of the movements of one hand while they are present in the movements of the other.

All of the disturbances, herein reported in the extra-speech musculature, occur at the time of the speech block. During stuttering not only is the speech mechanism proper blocked but also such extra-speech structures as the hands and feet. This would indicate that stuttering is due to or causes or accompanies a generalized motor blocking.

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PRACTICE IN RATING

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Thorndike [1] claims that there is a constant error in subjective ratings because the estimate of a specific trait may be influenced by the rater's general impression of the individual rated. He says that the magnitude of the constant error of the 'halo' seems surprisingly large, but that we have no way to measure it. It is possible that lack of experience may be a big factor in determining the size of this error. The majority of people might show a large error in estimating the price of airplanes, platinum, radium, and many other things of which they have little knowledge. Lack of training and experience likely enter to a great extent into the matter of the 'halo' effect.

The purpose of this experiment was to determine whether practice in rating would increase one's efficiency in such functions. For purposes of comparison two classes in educational psychology were chosen. Each class consisted chiefly of juniors and seniors, and the average daily attendance in each was from thirty-five to forty. One section was used as a control group; the other as a drill group.

It seemed improbable that people could be obtained for even one rating and still less likely that they could be induced to return for a second. For practice material, therefore, twenty victrola records were used. They consisted of various types of anthems, quartettes, and other musical selections, all of which were accompanied by some kind or kinds of musical instruments.

The selections were rated on the following six points which were defined in class and kept in the students' notebooks. 1. Volume, 2. Expression, 3. Quality, 4. Melody, 5. Har-

[1] Thorndike, E. L., A constant error in psychological ratings, *Journal of Applied Psychology*, 4 : 25–29, March, 1920.

mony, 6. Rhythm. The instructions were to rate each point on the basis of 100 percent. These six ratings were later averaged for the total rating of a given record.

At the beginning of the experiment the first three records were rated by both groups. After that the drill group was given new records three times a week, and also one or two of the records previously rated. The control group was given nothing more to rate except the first three records which were used again at the close of the experiment. In the drill group a great deal of discussing was done between ratings relative to methods, point of reference—whether to think of each rendition as perfect and mark down for errors, or whether to think of each one as being 50 percent or 75 percent perfect and mark up for good points, etc.

RELIABILITY COEFFICIENTS—DRILL GROUP

Record	Self-r	PE
1	.860	.028
2	.755	.046
3	.726	.051
4	.794	.044
5	.755	.050
6	.650	.068
7	.497	.090
8	.899	.023
9	.794	.044
10	.784	.045
11	.657	.062
12	.687	.058
13	.803	.041
14	.736	.055
15	.794	.045
16	.803	.043
17	.647	.066
18	.657	.063
19	.497	.088
20	.870	.028

RELIABILITY COEFFICIENTS OF THE CONTROL GROUP

Record	Self-r	PE
1	.343	.100
2	.364	.098
3	.628	.068

Twenty records were given twice each to the practice group and twenty reliability coefficients were estimated for the group. Three reliability coefficients were computed for the control group. Also the sigma and the coefficient of

variation were found for every rating of both groups. It was thought that an increase in the self-*r* and a decrease in the coefficient of variation would show improvement in rating ability. It was also supposed that if on the first three records the self-*r*'s of the practice group were greater than those of the control group this would give further evidence of the value of practice in increasing one's efficiency in rating. The statistical data are given here.

The correlations of the drill group show very little if any increase, but they are consistently higher than the self-*r*'s of the control group. The average of the PE's is also lower than that of the control group.

COEFFICIENTS OF VARIATION

Drill Group

Record	First C.V.	Second C.V.
1	.100	.130
2	.115	.093
3	.149	.185
4	.166	.142
5	.129	.129
6	.160	.090
7	.090	.082
8	.110	.096
9	.090	.093
10	.090	.098
11	.101	.086
12	.066	.068
13	.089	.089
14	.090	.096
15	.080	.070
16	.090	.090
17	.110	.107
18	.098	.098
19	.093	.086
20	.103	.106
Mean	.1059	.1017

Control Group

Record	First C.V.	Second C.V.
1	.076	.123
2	.099	.065
3	.134	.170
Mean	.103	.119

Three factors in the experiment may have prevented the reliability coefficients of the drill group from being consistently high; that is, high for all the records. One was that, although the number of raters was practically the same every

day, the individuals were not exactly the same. Due to absences there were sometimes from one to four changes in the group. A second factor was that a few of the raters seemed somewhat erratic, making as much as a forty percent change in two successive judgments. A third factor is related to the second. Some of the records were obviously not well liked by the students, and the more sensitive ones tended to change to a much lower mark on the second rendition of the objectionable selections. Of course, if actual use were to be made of such ratings, those of the most erratic logically would not be included in the totals nor in the averages. In general, the results of the experiment seem to indicate that by training in subjective ratings individuals may improve; that their ratings may more nearly agree and that they may become more reliable.

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AN AUTOMATIC ROTARY SWITCH FOR USE WITH THE RANSCHBURG EXPOSURE APPARATUS FOR CONTINUOUS MULTIPLE CHOICE WORK

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The Ranschburg Exposure Apparatus lends itself to a variety of uses in conjunction with other extremely simple apparatus for the study of serial multiple choice reactions demanding discrimination. Essentially this instrument consists of a rotating carriage upon which is mounted a cardboard disc so that radial segments of the disc appear successively just beneath an aperture in the cover of the instrument. The rotation of the carriage is effected by means of an electromagnet whose armature, when drawn toward the poles of the magnet, actuates a pawl and ratchet mechanism which controls the extent of rotation of a toothed wheel and shaft upon which this carriage is mounted. By means of two contacts another circuit, the external circuit, may thereby be closed when the armature moves over to the extremity of its throw.

Professor Dashiell [1] has described one use of the Ranschburg Apparatus for serial choice reactions. Three telegraph keys were placed parallel in the internal circuit of the Ranschburg with corresponding signal markers so that a kymograph record was obtained and the exposure disc of the Ranschburg was actuated to move so as to expose a new symbol when either right or wrong key was pressed. The inadequacies of such an arrangement are quite apparent. For instance, it permitted detection of erroneous responses only by comparison of the record with the stimulus series. An independent time record for correct and incorrect responses respectively was not obtainable. Moreover, the speed of presentation of

[1] J. F. Dashiell, Some simple apparatus for serial reactions. *Journal of Experimental Psychology*, 1930, 13, 352–357.

successive stimuli was governed exclusively by the reaction time of the subject regardless of whether or not the ensuing responses were right or wrong. Other forms of multiple choice apparatus using the Ranschburg instrument impose upon the experimenter the necessity of directing his attention to the manipulation and adjustment of apparatus during the conduct of the experiment and to the re-setting of the instrument upon the correct solution of a problem or a short series of problems.

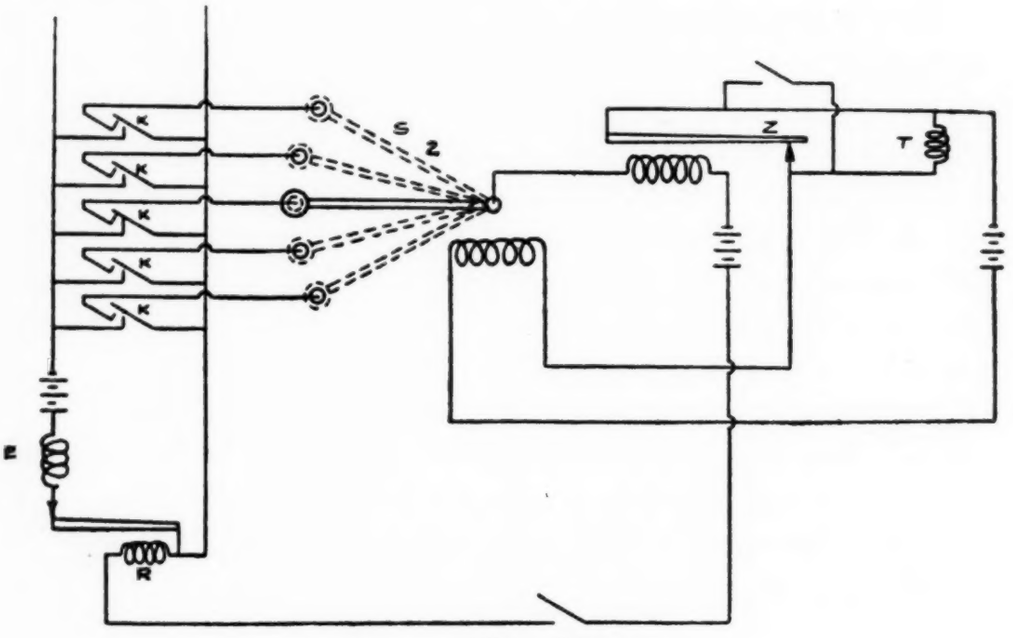

FIG. 1. Wiring Diagram

CODE

E = Error Signal Marker
K = Keys
R = Relay
S = Switch
T = True Response Marker
Z = Armature of Ranschburg

To eliminate the disadvantages above mentioned, and at the same time to permit instrumental recording of time, errors, and correct responses, when used with signal markers or with a polygraph, an automatic rotary switch was constructed by the author and has been successfully used in conjunction with the Ranschburg apparatus for continuous multiple choice work.

Essentially this switch consists of a rotating brush which moves over a series of insulated segments corresponding in number to the sectors of the Ranschburg exposure card (Fig. 2). Contact of the brush with a given segment of the switch closes a circuit through the electromagnets of the Ranschburg when the key corresponding to that segment is depressed. The brush is driven by means of an electromagnet whose armature actuates a pawl and ratchet mechanism which rotates a shaft to which the brush is attached.

Incidental to his setting up of apparatus for measuring the effect of certain distractions upon habit integration the writer required an automatic switch with fifty contact points to correspond with each fifty sectors on the exposure card of the Ranschburg. Upon the exposure of a certain symbol, the subject was required to depress the corresponding one of twelve keys. The order of occurrence of these twelve symbols in an entire series of fifty was determined by chance and the symbols were printed upon the exposure card in this order. The segments of the rotary switch were then connected with their corresponding keys and the Ranschburg Apparatus was placed in the circuit as indicated in the diagram.

The switch itself was constructed from an electric counter, Stœlting's Model No. 22407 (Fig. 3). The hands and dial of the counter were first removed and a 5 mm hole was drilled at the center of a celluloid disc 10 cm in diameter and 2 mm thick. This disc was then fitted over the upright shaft of the counter which projected after the hands and lock-nut were removed, and was securely fastened to the frame of the counter in the same way as was the original dial. It was then given two coats of shellac. A circular sheet of a heavy grade of lead foil, 10 cm in diameter, was then fastened firmly to the disc. The rotor of the switch—a rotating arm and brush—was then mounted upon the upright shaft formerly carrying the hands of the counter. A strip of brass, 6 cm in length and 1½ cm in width, was soldered to the lock-nut which was then screwed onto the threaded end of the upright shaft. A satisfactory brush was made from a fountain pen clip which was cut off and soldered at right angles to the arm of the switch and the sur-

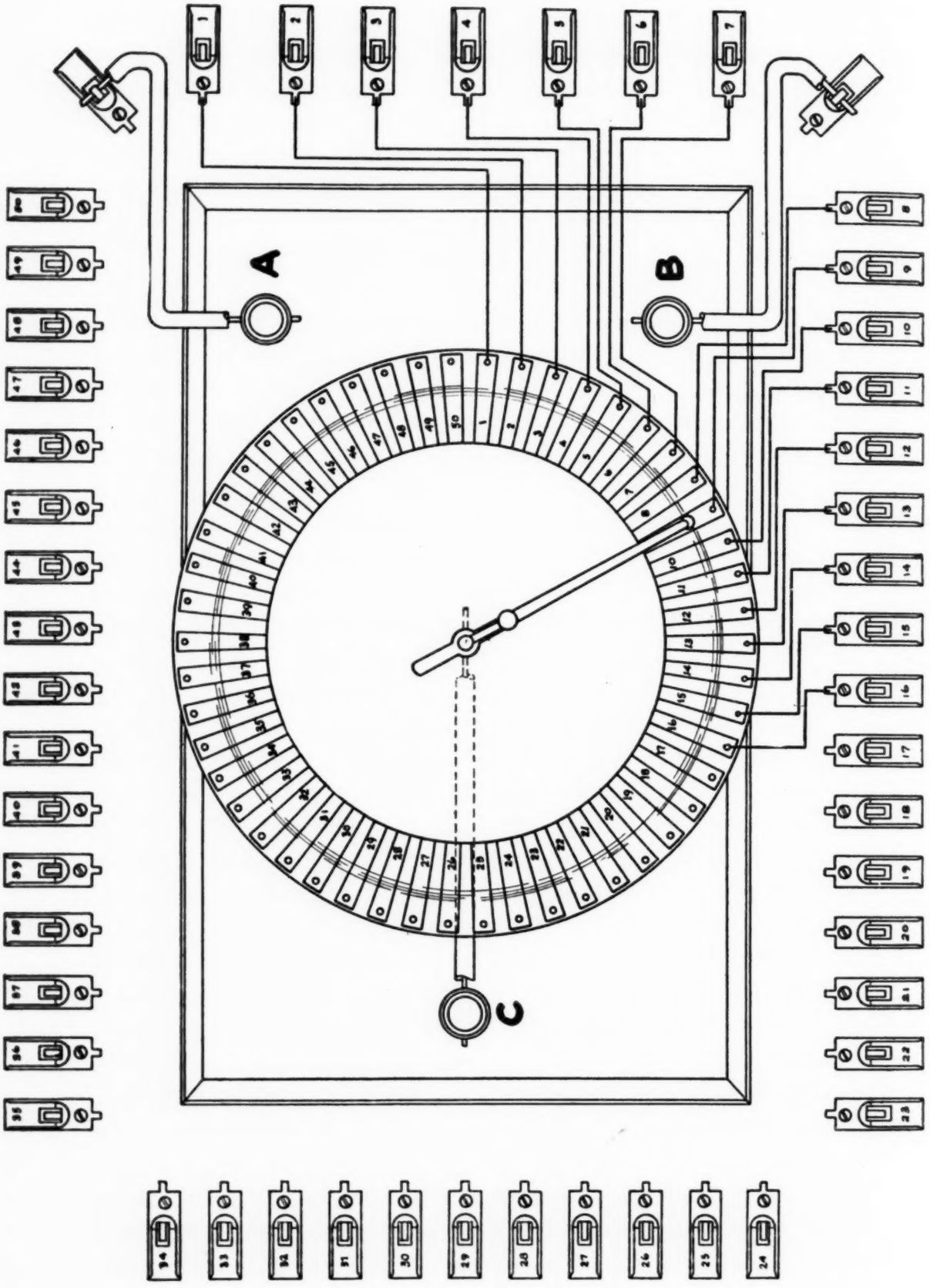

FIG. 2. Top View of Rotary Switch

face of the lead foil so that the ball-like end of the clip bore with some pressure against the foil-covered dial.

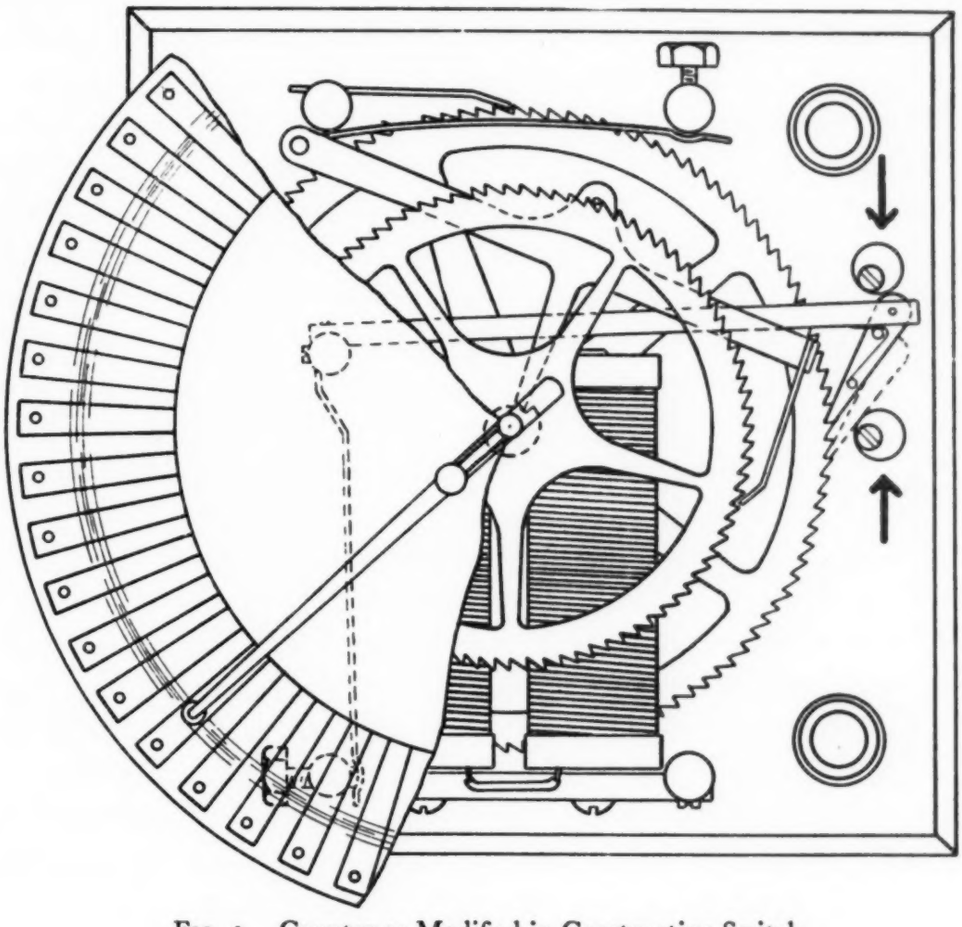

FIG. 3. Counter as Modified in Constructing Switch

The movement of the hands across the dial of the original counter was effected by means of the upright shaft to which they were fastened. At the lower extremity of this shaft was rigidly fastened a cogwheel with 100 teeth. The rotation of this wheel caused a rotation of the shaft and the hands attached to it. This cogwheel was driven by a pawl which engaged the teeth of the wheel when the armature of an electromagnet was drawn towards its poles. The pawl and ratchet mechanism of the counter may be readily adjusted so that the pawl engages every other tooth of the wheel thus doubling the distance that the hands will move on the dial. Such an adjustment was necessary for our purpose for the distance between successive points on the dial must be sufficient to

FIG. 2. Top View of Rotary Switch

permit the insertion of contact strips well insulated from each other.

When a circuit through the instrument was closed the arm and brush moved over the surface of the foil-covered dial a distance equal to 1/50 of the circumference of the dial. These successive points were then marked and at a distance of ¼ cm on each side the foil was cut away radially leaving fifty segments ½ cm wide insulated from each other. A coat of shellac and a disc of bakelite 6 cm in diameter with a 5 mm hole at its center were applied to the center of the dial and segments. To the peripheral end of each segment was soldered a 15 cm lead of No. 28 copper wire. The whole instrument was then mounted upon a substantial base 30 cm × 40 cm and 53 binding posts were inserted in the base of the instrument. Fifty of these received the leads from the switch segments. The posts marked *A* and *B* (Fig. 2) are the terminations of the leads from the electromagnets of the counter. The post marked *C* is the termination of the lead soldered to the frame of the instrument and it serves as a ground.

The subject's twelve keys were placed in circuit with the Ranschburg Apparatus and the rotary switch. See Fig. 1. Flexible leads from each of the twelve keys were connected to the appropriate switch segments in accordance with the succession of symbols on the exposure card.

The problem demanded an automatic record of time, errors, and correct responses. This was obtained by utilizing three of the signal markers of the Renshaw Polygraph. Double contact keys were used. Pressure on an incorrect key, *e.g.*, No. 2 in the figure, was recorded through circuit 'X' operating an error signal marker, 'E', the circuit through the Ranschburg being broken from that key at the switch *S*. Pressure on the correct key, *e.g.*, No. 3 in the figure, closed the internal Ranschburg circuit, connection for that key being already established at the switch *S*, and a relay *R* in this circuit broke the 'error' circuit. Meanwhile, the closing of the internal circuit effected movement of the armature *Z* of the Ranschburg, thus bringing a new symbol under the aperture; and, by closing the external circuit simultaneously

operating the true response marker *T* and re-setting the switch *S*. Time was indicated on the record by means of an independent circuit and signal marker, thus enabling us to obtain separate determinations for the correct and the erroneous responses.

The resistance of the electro-magnets of the switch was found to be approximately 60 ohms, therefore, the use of a 24 volt source of supply for the switch is recommended.

The rotary switch above described may be constructed from materials ordinarily available in the laboratory shop at a cost not in excess of $5.00. It should prove a serviceable addition to the equipment of any laboratory, for it may be used for controlling the operation of various electrically driven apparatus.

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